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CONTENTS

THE PRESIDENT'S ADDRESS

By Sir T. Edward Thorpe, C.B., D.Sc., Sc.D., LL.D., F.R.S.

ADDRESSES OF THE PRESIDENTS OF SECTIONS

Problems of Physics . . .	**By Prof. O. W. Richardson, D.Sc., F.R.S.**
The Laboratory of the Living Organism	**By M. O. Forster, D.Sc., F.R.S.**
Experimental Geology . . .	**By J. S. Flett, D.Sc., LL.D., F.R.S.**
Some Problems in Evolution . .	**By Prof. E. S. Goodrich, M.A., F.R.S.**
The Application of Geography . .	**By D. G. Hogarth, C.M.G., Litt.D.**
The Principles by which Wages are Determined	**By W. L. Hichens, M.A.**
Water Power	**By Prof. A. H. Gibson, D.Sc.**
The Boundaries of Physiology . .	**By Sir Walter M. Fletcher, K.B.E., M.D., Sc.D., F.R.S.**
Consciousness and the Unconscious.	**By Prof. C. Lloyd Morgan, LL.D., D.Sc., F.R.S.**
The Present Position of the Theory of Descent, in Relation to the Early History of Plants	**By D. H. Scott, D.Sc., LL.D., F.R.S.**
The Place of Music in a Liberal Education	**By Sir Henry Hadow, C.B.E., D.Mus.**
The Study of Agricultural Economics	**By C. S. Orwin, M.A.**

ADDRESS TO THE CONFERENCE OF DELEGATES OF CORRESPONDING SOCIETIES

The Message of Science . . .	**By Sir Richard Gregory.**

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THE

PRESIDENTIAL ADDRESS,

BY

Sir T. EDWARD THORPE, C.B., D.Sc., Sc.D., LL.D., F.R.S., Hon. F.R.S., Edin.,

PRESIDENT OF THE ASSOCIATION.

THE British Association for the Advancement of Science owes its origin, and, in great measure, its specific aims and functions, to the public spirit and zeal for the interests of science of Scotsmen. Its virtual founder was Sir David Brewster; its scope and character were defined by Principal Forbes. In constitution it differed from the migratory scientific associations existing on the Continent, which mainly served to promote the social intercourse of their members by annual gatherings, in that it was to be a permanent organisation, with a settled establishment and headquarters, which should have not merely its yearly reunions, but which, 'by methods and by influence peculiarly its own, should continue to operate during the intervals of these public assemblies, and should aspire to give an impulse to every part of the scientific system; to mature scientific enterprise; and to direct the labours requisite for discovery.'

Although, for reasons of policy, it was decided that its first meeting of September 27, 1831, should be held at York, as the most central city for the three kingdoms, and its second and third meetings at the ancient Universities of Oxford and Cambridge respectively, it was inevitable that the Association should seize the earliest opportunity to visit the Metropolis of Scotland where, as an historical fact, it may be said to have had its origin.

The meeting in this city of September 8, 1834, was noteworthy for many reasons. It afforded the first direct proof that the Association was fulfilling its purpose. This was shown by the popular appreciation which attended its activities, by the range and character of its reports on the state and progress of science, by the interest and value of its sectional proceedings, and by the mode in which its funds were employed. In felicitous terms the President of the preceding year, the Rev. Professor Sedgwick, congratulated the gathering 'on the increased strength in which they had assembled, in a place endeared to the feelings of every lover of science by so many delightful and elevating

recollections, especially by the recollection of the great men whom it had fostered, or to whom it had given birth.' In a few brief sentences Professor Sedgwick indicated the great power which this Association is able to apply towards the advancement of science by combination and united action, and he supported his argument by pointing to the results which it had already achieved during the three short years of its existence. Professor Sedgwick's words are no less true to-day. His contention that one of the most important functions of this philosophical union is to further what he termed the 'commerce of ideas' by joint discussions on subjects of kindred interest, has been endorsed by the recent action of the Council in bringing the various sections into still closer touch with each other with a view to the discussion of common problems of general interest. This slight reorganisation of the work of the Sections, which is in entire accord with the spirit and aims of the Association, as defined by its progenitors and formulated in its constitution, will take effect during the present meeting. Strictly speaking, such joint sectional discussions are not unknown in our history, and their utility and influence have been freely recognised. But hitherto the occasions have been more or less informal. They are now, it is hoped, to be part of the regular official procedure of the meetings, to which it is anticipated they will afford additional interest and value.

Another noteworthy change in our procedure is the introduction of discussions on the addresses of the Presidents of Sections. Hitherto these addresses have been formally read and never discussed. To the extent that they have been brief chronicles of the progress of the special departments of science with which the section is concerned they have given but little opportunity for discussion. With the greatly increased facilities which now exist for every worker to keep himself informed of the development of the branch of knowledge in which he is more particularly interested, such *résumés* have in great measure lost their true purpose, and there has, consequently, been a growing tendency of late years for such presidential addresses to deal with contemporary topics of general interest and of fundamental importance, affording ample opportunity for a free exchange of opinion. The experiment will certainly conduce to the interest of the proceedings of the sections, and will contribute to the permanent value of their work. We see in these several changes the development of ideas connected with the working of the Association which may be said to have had their birth at its first meeting in Edinburgh, eighty-seven years ago.

Sixteen years later, that is on July 21, 1850, Edinburgh again extended her hospitality to the British Association, which then honoured itself by electing the learned Principal of the United Colleges of St. Salvator and St. Leonard, St. Andrews, to the presidential chair—at once a tribute to Sir David Brewster's eminence as a natural philo-

sopher, and a grateful recognition of his services to this body in suggesting and promoting its formation.

On the occasion of his inaugural address, after a brief account of recent progress in science, made with the lucidity of expression which characterised all the literary efforts of the learned biographer of Newton and versatile editor of the *Edinburgh Encyclopædia*, the *Edinburgh Magazine*, and the *Edinburgh Journal of Science*, the President dwelt upon the beneficent influence of the Association in securing a more general attention to the objects of science, and in effecting a removal of disadvantages of a public kind that impeded its progress. It was largely to the action of the Association, assisted by the writings and personal exertions of its members, that the Government was induced to extend a direct national encouragement to science and to aid in its organisation.

Brewster had a lofty ideal of the place of science in the intellectual life of a community, and of the just position of the man of science in the social scale. In well-weighed words, the outcome of matured experience and of an intimate knowledge of the working of European institutions created for the advancement of science and the diffusion of knowledge, he pleaded for the establishment of a national institution in Britain, possessing a class of resident members who should devote themselves wholly to science—with a place and station in society the most respectable and independent—'free alike,' as Playfair put it, 'from the embarrassments of poverty or the temptations of wealth.' Such men, 'ordained by the State to the undivided functions of science,' would, he contended, do more and better work than those who snatch an hour or two from their daily toil or nightly rest.

This ideal of 'combining what is insulated, and uniting in one great institution the living talent which is in active but undirected and unbefriended exercise around us,' was not attained during Brewster's time; nor, notwithstanding the reiteration of incontrovertible argument during the past seventy years, has it been reached in our own.

I have been led to dwell on Sir David Brewster's association with this question of the relations of the State towards research for several reasons. Although he was not the first to raise it—for Davy more than a century ago made it the theme of presidential addresses, and brought his social influence to bear in the attempt to enlist the practical sympathy of the Government—no one more consistently urged its national importance, or supported his case with a more powerful advocacy, than the Principal of the University of Edinburgh. It is only seemly, therefore, that on this particular occasion, and in this city of his adoption, where he spent so much of his intellectual energy, I should specially allude to it. Moreover, we can never forget what this Association owes to his large and fruitful mind. Every man is a

debtor to his profession, from which he gains countenance and profit. That Brewster was an ornament to his is acknowledged by every lover of learning. That he endeavoured to be a help to it was gratefully recognised during his lifetime. After his death it was said of him that the improved position of men of science in our time is chiefly due to his exertions and his example.

I am naturally led to connect the meeting of 1850 with a still more memorable gathering of this Association in this city. In August 1871—just over half a century ago—the British Association again assembled in Edinburgh under the presidency of Lord Kelvin—then Sir William Thomson. It was an historic occasion by reason of the address which inaugurated its proceedings. Lord Kelvin, with characteristic force and insistence, still further elaborated the theme which had been so signal a feature of Sir David Brewster's address twenty years previously: 'Whether we look to the honour of England,' he said, 'as a nation which ought always to be the foremost in promoting physical science, or to those vast economical advantages which must accrue from such establishments, we cannot but feel that experimental research ought to be made with us an object of national concern, and not left, as hitherto, exclusively to the private enterprise of self-sacrificing amateurs, and the necessarily inconsecutive action of our present Governmental Departments and of casual committees.'

Lord Kelvin, as might have been anticipated, pleaded more especially for the institution of physical observatories and laboratories for experimental research, to be conducted by qualified persons, whose duties should be not teaching, but experimenting. Such institutions as then existed, he pointed out, only afforded a very partial and inadequate solution of a national need. They were, for the most part, 'absolutely destitute of means, material, or *personnel* for advancing science, except at the expense of volunteers, or of securing that volunteers should be found to continue such little work as could then be carried on.'

There were, however, even then, signs that the bread cast upon the waters was slowly returning after many days. The establishment of the Cavendish Laboratory at Cambridge, by the munificence of its then Chancellor, was a notable achievement. Whilst in its constitution as part of a university discipline it did not wholly realise the ideal of the two Presidents, under its successive directors, Prof. Clerk-Maxwell, the late Lord Rayleigh, and Sir J. J. Thomson, it has exerted a profound influence upon the development of experimental physics, and has inspired the foundation of many similar educational institutions in this country. Experimental physics has thus received an enormous impetus during the last fifty years, and although in matters of science there is but little folding of the hands to sleep, 'the divine discontent' of its followers

has little cause for disquietude as regards the position of physics in this country.

In the establishment of the National Physical Laboratory we have an approach to the ideal which my predecessors had so earnestly advocated. Other Presidents, among whom I would specially name the late Sir Douglas Galton, have contributed to this consummation. The result is a remarkable testimony to the value of organised and continuous effort on the part of the British Association in forming public opinion and in influencing Departmental action. It would, however, be ungrateful not to recall the action of the late Lord Salisbury—himself a follower of science and in full sympathy with its objects—in taking the first practical steps towards the creation of this magnificent national institution. I may be allowed, perhaps, to refer to this matter, as I have personal knowledge of the circumstances, being one of the few survivors of the Committee which Lord Salisbury caused to be formed, under the chairmanship of the late Lord Rayleigh, to inquire and report upon the expediency of establishing an institution in Great Britain upon the model of certain State-aided institutions already existing on the Continent, for the determination of physical constants of importance in the arts, for investigations in physical problems bearing upon industry, for the standardisation and verification of physical instruments, and for the general purposes of metrology. I do not profess to give the exact terms of the reference to the Committee, but, in substance, these were recognised to be the general aims of the contemplated institute. The evidence we received from many men of science, from Departmental officers, and from representatives of engineering and other industrial establishments was absolutely unanimous as to the great public utility of the projected laboratory. It need hardly be said that the opportunity called forth all the energy and power of advocacy of Lord Kelvin, and I well remember with what strength of conviction he impressed his views upon the Committee. That the National Physical Laboratory has, under the ability, organising power, and business capacity of its first director, Sir Richard Glazebrook, abundantly justified its creation is recognised on all hands. Its services during the four years of war alone are sufficient proof of its national value. It has grown to be a large and rapidly increasing establishment, occupying itself with an extraordinary range of subjects, with a numerous and well-qualified staff, engaged in determinative and research work on practically every branch of pure and applied physics. The range of its activities has been further increased by the establishment since the war of co-ordinating research boards for physics, chemistry, engineering and radio-research. Government Departments have learned to appreciate its services. The photometry division, for example, has been busy on experiments on navigation lamps for the

Board of Trade, on miners' lamps for the Home Office and on motor-car head-lamps for the Ministry of Transport, and on the lighting of the National Gallery and the Houses of Parliament. Important work has been done on the forms of ships, on the steering and manœuvring of ships, on the effect of waves on ship resistance, on the interaction between passing ships, on seaplane floats, and on the hulls of flying-boats.

It is also actively engaged in the study of problems connected with aviation, and has a well-ordered department for aerodynamical research.

It can already point to a long and valuable series of published researches, which are acknowledged to be among the most important contributions to pure and applied physics which this country has made during recent years.

I may be pardoned, I hope, for another personal reference, if I recall that it was at the Edinburgh meeting, under Lord Kelvin's presidency, fifty years ago, that I first became a member of this Association, and had the honour of serving it as one of the secretaries of its chemical section. Fifty years is a considerable span in the life of an individual, but it is a relatively short period in the history of science. Nevertheless, those fifty years are richer in scientific achievement and in the importance and magnitude of the utilitarian applications of practically every branch of science than any preceding similar interval. The most cursory comparison of the state of science, as revealed in his comprehensive address, with the present condition of those departments on which he chiefly dwelt, will suffice to show that the development has been such that even Lord Kelvin's penetrative genius, vivid imagination, and sanguine temperament could hardly have anticipated. No previous half-century in the history of science has witnessed such momentous and far-reaching achievements. In pure chemistry it has seen the discovery of argon by Rayleigh, of radium by Madame Curie, of helium as a terrestrial element by Ramsay, of neon, xenon, and krypton by Ramsay and Travers, the production of helium from radium by Ramsay and Soddy, and the isolation of fluorine by Moissan. These are undoubtedly great discoveries, but their value is enormously enhanced by the theoretical and practical consequences which flow from them.

In applied chemistry it has witnessed the general application of the Gilchrist-Thomas process of iron-purification, the production of calcium cyanamide by the process of Frank and Caro, Sabatier's process of hydrogenation, a widespread application of liquefied gases, and Haber's work on ammonia synthesis—all manufacturing processes which have practically revolutionised the industries with which they are concerned.

In pure physics it has seen the rise of the electron theory, by

Lorentz; Hertz's discovery of electro-magnetic waves; the investigation of cathode rays by Lenard, and the elucidation of crystal structure by Bragg.

It has seen, moreover, the invention of the telephone, the establishment of incandescent lighting, the electric transmission of force, the invention of the cinematograph, of wireless telegraphy, the application of the Röntgen rays, and the photographic reproduction of colour.

In physical chemistry it has witnessed the creation of stereochemistry by Van t'Hoff and Le Bel, Gibbs' work on the phase rule, Van t'Hoff's theory of solutions, Arrhenius's theory of ionic dissociation, and Nernst's theory of the galvanic cell.

Such a list is far from complete, and might be greatly extended. But it will at least serve to indicate the measure of progress which the world owes to the development and application during the last fifty years of the two sciences—physics and chemistry—to which Lord Kelvin specially referred.

The more rapid dissemination of information concerning the results of recent or contemporary investigation, which Lord Kelvin so strongly urged as 'an object to which the powerful action of the British Association would be thoroughly appropriate,' has been happily accomplished. The timely aid of the Association in contributing to the initial expense of preparing and publishing monthly abstracts of foreign chemical literature by the Chemical Society is gratefully remembered by British chemists. The example has been followed by the greater number of our scientific and technical societies, and the results of contemporary inquiry in every important branch of pure and applied science are now quickly brought to the knowledge of all interested workers. In fact, as regards the particular branch of science with which I am more directly concerned, the arrangements for the preparation and dissemination of abstracts of contemporary foreign chemical literature are proving to be a veritable embarrassment of riches, and there is much need for co-operation among the various distributing societies. This need is especially urgent at the present time owing to the greatly increased cost of paper, printing, binding, and indeed of every item connected with publication, which expense, of course, ultimately falls upon the various societies and their members. The problem, which has already received some attention from those entrusted with the management of the societies referred to, is not without its difficulties, but these are not insoluble. There is little doubt that a resolute and unanimous effort to find a solution would meet with success.

The present high cost of book production, which in the case of specialised books is about three times what it was in 1914, is exercising a most prejudicial effect upon the spread of scientific knowledge. Books on science are not generally among the 'best sellers.' They appeal to a

comparatively limited and not particularly wealthy public, largely composed of the professional classes who have suffered in no small measure from the economic effects of the War. The present high price of this class of literature is to the public detriment. Eventually it is no less to the detriment of the printing and publishing trades. Publishers are well aware of this fact, and attempts are being made by discussions between employers and the executives of the Typographical Association and other societies of compositors to reach an equitable solution, and it is greatly to be hoped that it will be speedily found.

All thinking men are agreed that science is at the basis of national progress. Science can only develop by research. Research is the mother of discovery, and discovery of invention. The industrial position of a nation, its manufactures and commerce, and ultimately its wealth, depend upon invention. Its welfare and stability largely rest upon the equitable distribution of its wealth. All this seems so obvious, and has been so frequently and so convincingly stated, that it is superfluous to dwell upon it in a scientific gathering to-day.

A late distinguished Admiral, you may remember, insisted on the value of reiteration. On this particular question it was never more needed than now. The crisis through which we have recently passed requires it in the interests of national welfare. Of all post-war problems to engage our serious attention, none is more important in regard to our position and continued existence than the nation's attitude towards science and scientific research, and there is no more opportune time than the present in which to seek to enforce the teaching of one of the most pregnant lessons of our late experience.

It is, unfortunately, only too true that the industrial world has in the past underrated the value of research. One indication that the nation is at length aroused to its importance is to be seen in the establishment of the Department of Scientific and Industrial Research, with its many subordinate associations. The outbreak of the Great War, and much in its subsequent history, revealed, as we all know, many national shortcomings, due to our indifference to and actual neglect of many things which are at the root of our prosperity and security. During the War, and at its close, various attempts, more or less unconnected, were made to find a remedy. Of the several committees and boards which were set up, those which still exist have now been co-ordinated, and brought under the control of a central organisation—the Department of Scientific and Industrial Research. Research has now become a national and State-aided object. For the first time in our history its pursuit with us has been organised by Government action. As thus organised it seeks to fulfil the aspirations to which I have referred, whilst meeting many of the objections which have been urged against the endowment of research. It must be recognised that modern

ideas of democracy are adverse to the creation of places to which definite work is not assigned and from which definite results do not emanate. This objection, which strikes at the root of the establishment of such an institution as Sir David Brewster contemplated, is, to a large extent, obviated by the scheme of the Department of Scientific and Industrial Research. *It does not prescribe or fetter research*, but, whilst aiding by personal payments the individual worker, leaves him free to pursue his inquiry as he thinks best. Grants are made, on the recommendation of an Advisory Council of experts, to research workers in educational institutions and elsewhere, in order to promote research of high character on fundamental problems of pure science or in suitable cases on problems of applied science. Of the boards and committees and similar organisations established prior to or during the War, or subsequent to it, with one or two exceptions, all are now directly under the Department. They deal with a wide range of subjects, such as the Building Research Board, established early in 1920 to organise and supervise investigations on building materials and construction, to study structural failures, and to fix standards for structural materials. The Food Investigation Board deals with the preservation by cold of food, and with the engineering problems of cold storage, with the chemistry of putrefaction, and the agents which induce it, with the bionomics of moulds, and the chemistry of edible oils and fats. The Fuel Research Board is concerned with the immediate importance of fuel economy and with investigations of the questions of oil-fuel for the Navy and Mercantile Marine, the survey of the national coal resources, domestic heating, air pollution, pulverised fuel, utilisation of peat, the search for possible substitutes for natural fuel oil, and for practicable sources of power alcohol.

The Geological Survey Board has taken over the Geological Survey of Great Britain and the control of the Museum of Practical Geology. The maintenance of the National Physical Laboratory, originally controlled by a General Board and an Executive Committee appointed by the President and Council of the Royal Society, is now transferred to the Department of Scientific and Industrial Research. A Mines Research Committee and a Mine Rescue Apparatus Committee are attached to the Department. The former is concerned with such questions as the determination of the geothermic gradient, the influence of temperature of intake and return air on strata, the effect of seasonal changes on strata temperature of intakes, the cooling effect due to the evolution of fire-damp, heat production from the oxidation of timber, etc. The Department is also directing inquiries on the preservation and restoration of antique objects deposited in the British Museum. It is concerned with the gauging of rivers and tidal currents, with special reference to a hydrographical survey of Great Britain in relation to

the national resources of water-power. In accordance with the Government policy, four co-ordinating boards have been established to organise scientific work in connection with the fighting forces, so as to avoid unnecessary overlapping and to provide a single direction and financial control. The four boards deal, respectively, with chemical and physical problems, problems of radio-research, and engineering. These boards have attached to them various committees dealing with special inquiries, some of which will be carried out at the National Physical Laboratory. The Government have also authorised the establishment of a Forest Products Research Board.

The Department is further empowered to assist learned or scientific societies and institutions in carrying out investigations. Some of these were initiated prior to the War, and were likely to be abandoned owing to lack of funds. Whenever the investigation has a direct bearing upon a particular industry that had not hitherto been able to establish a Research Association, it has been a condition of a grant that the institution directing the research should obtain contributions towards the cost on a £ for £ basis, either directly through its corporate funds or by special subscriptions from interested firms. On the formation of the appropriate association the research is, under suitable safeguards, transferred to it for continuance. The formation of a number of Research Associations has thus been stimulated, dealing, for example, with scientific instruments, non-ferrous metals, glass, silk, refractories, electrical and allied industries, pottery, etc.

Grants are made to Research Associations formed voluntarily by manufacturers for the purposes of research, from a fund of a million sterling, placed at the disposal of the Research Department for this purpose. Such Associations, to be eligible for the grant, must submit Articles of Association for the approval of the Department and the Board of Trade. If these are approved, licences are issued by the Board of Trade recognising the Associations as limited liability companies working without profits. Subscriptions paid to an Association by contributing firms are recognised by the Board of Inland Revenue as business costs of the firms, and are not subject to income or excess profits taxes. The income of the Association is similarly free of income tax. Grants are ordinarily made to these Associations on the basis of £1 for every £1 raised by the Association between limits depending upon the particular industry concerned. In the case of two Research Associations grants are made at a higher rate than £ for £, as these industries are regarded as having a special claim to State assistance on account of their 'pivotal' character. The results of research are the sole property of the Association making them, subject to certain rights of veto possessed by the Department for the purposes of ensuring that they are not communicated to foreign countries, except with the consent

of the Department, and that they may be made available to other interested industries and to the Government itself on suitable terms.

These arrangements have been found to be generally satisfactory, and at the present time twenty-four of such Research Associations have been formed to whom licences have been issued by the Board of Trade. Others are in process of formation, and may be expected to be at work at an early date. These Research Associations are concerned with nearly all our leading industries. The official addresses of most of them are in London; others have their headquarters in Manchester, Leeds, Sheffield, Birmingham, Northampton, Coventry, Glasgow, and Belfast.

The Department has further established a Records Bureau, which is responsible for receiving, abstracting, filing and collating communications from research workers, boards, institutions, or associations related to or supervised by the Department. This information is regarded as confidential, and will not be communicated except in writing, and after consultation with the research worker or organisation from which it has been received. Also such non-confidential information as comes into the possession of the Department which is of evident or probable value to those working in touch with the Department is collected and filed in the Bureau and made generally available.

It is also a function of the Bureau to effect economy in preventing repetition and overlapping of investigations and in ensuring that the fullest possible use is made of the results of research. Thus, the programmes of Research Associations are compared in order to ensure that researches are not unwittingly duplicated by different Research Associations. Sometimes two or more Research Associations may be interested in one problem from different points of view, and when this occurs it may be possible for the Bureau to arrange a concerted attack upon the common problem, each Research Association undertaking that phase of the work in which it is specially interested and sharing in the general results.

As researches carried out under the Department frequently produce results for which it is possible to take out patents, careful consideration has been given to the problems of policy arising on this subject, and other Government Departments also interested have been freely consulted. As the result, an Inter-Departmental Committee has been established with the following terms of reference:—

(1) To consider the methods of dealing with inventions made by workers aided or maintained from public funds, whether such workers be engaged (*a*) as research workers, or (*b*) in some other technical capacity, so as to give a fair reward to the inventor and thus encourage further effort, to secure the utili-

sation in industry of suitable inventions and to protect the national interest, and

(2) To outline a course of procedure in respect of inventions arising out of State-aided or supported work which shall further these aims and be suitable for adoption by all Government Departments concerned.

About forty patents have been taken out by the Department jointly with the inventors and other interested bodies, but of these, nine have subsequently been abandoned. At least five patents have been developed to such a stage as to be ready for immediate industrial application.

It will be obvious from this short summary of the activities of the Department, based upon information kindly supplied to me by Sir Francis Ogilvie, that this great scheme of State-aided research has been conceived and is administered on broad and liberal lines. A considerable number of valuable reports from its various boards and committees have already been published, and others are in the press, but it is, of course, much too soon to appreciate the full effects of their operations. But it can hardly be doubted that they are bound to exercise a profound influence upon industries which ultimately depend upon discovery and invention. The establishment of the Department marks an epoch in our history. No such comprehensive organisation for the application of science to national needs has ever been created by any other State. We may say we owe it directly to the Great War. Even from the evil of that great catastrophe there is some soul of goodness would we observingly distil it out.

I turn now to a question of scientific interest which is attracting general attention at the present time. It is directly connected with Lord Kelvin's address fifty years ago.

The molecular theory of matter—a theory which in its crudest form has descended to us from the earliest times and which has been elaborated by various speculative thinkers through the intervening ages, hardly rested upon an experimental basis until within the memory of men still living. When Lord Kelvin spoke in 1871, the best-established development of the molecular hypothesis was exhibited in the kinetic theory of gases as worked out by Joule, Clausius, and Clerk-Maxwell. As he then said, no such comprehensive molecular theory had ever been even imagined before the nineteenth century. But, with the eye of faith, he clearly perceived that, definite and complete in its area as it was, it was 'but a well-drawn part of a great chart, in which all physical science will be represented with every property of matter shown in dynamical relation to the whole. The prospect we now have of an early completion of this chart is based on the assumption of atoms. But there can be no permanent satisfaction to the mind in explaining

heat, light, elasticity, diffusion, electricity and magnetism, in gases, liquids and solids, and describing precisely the relations of these different states of matter to one another by statistics of great numbers of atoms when the properties of the atom itself are simply assumed. When the theory, of which we have the first instalment in Clausius and Maxwell's work, is complete, we are but brought face to face with a superlatively grand question: What is the inner mechanism of the atom?'

If the properties and affections of matter are dependent upon the inner mechanism of the atom, an atomic theory, to be valid, must comprehend and explain them all. There cannot be one kind of atom for the physicist and another for the chemist. The nature of chemical affinity and of valency, the modes of their action, the difference in characteristics of the chemical elements, even their number, internal constitution, periodic position, and possible isotopic rearrangements must be accounted for and explained by it. Fifty years ago chemists, for the most part, rested in the comfortable belief of the existence of atoms in the restricted sense in which Dalton, as a legacy from Newton, had imagined them. Lord Kelvin, unlike the chemists, had never been in the habit of 'evading questions as to the hardness or indivisibility of atoms by virtually assuming them to be infinitely small and infinitely numerous.' Nor, on the other hand, did he realise, with Boscovich, the atom 'as a mystic point endowed with inertia and the attribute of attracting or repelling other such centres.' Science advances not so much by fundamental alterations in its beliefs as by additions to them. Dalton would equally have regarded the atom 'as a piece of matter of measureable dimensions, with shape, motion, and laws of action, intelligible subjects of scientific investigation.'

In spite of the fact that the atomic theory, as formulated by Dalton, has been generally accepted for nearly a century, it is only within the last few years that physicists have arrived at a conception of the structure of the atom sufficiently precise to be of service to chemists in connection with the relation between the properties of elements of different kinds, and in throwing light on the mechanism of chemical combination.

This further investigation of the 'superlatively grand question—the inner mechanism of the atom,'—has profoundly modified the basic conceptions of chemistry. It has led to a great extension of our views concerning the real nature of the chemical elements. The discovery of the electron, the production of helium in the radioactive disintegration of atoms, the recognition of the existence of isotopes, the possibility that all elementary atoms are composed either of helium atoms or of atoms of hydrogen and helium, and that these atoms, in their turn, are built up of two constituents, one of which is the electron, a particle of

negative electricity whose mass is only 1/1800 of that of an atom of hydrogen, and the other a particle of positive electricity whose mass is practically identical with that of the same atom—the outcome, in short, of the collective work of Soddy, Rutherford, J. J. Thomson, Collie, Moseley and others—are pregnant facts which have completely altered the fundamental aspects of the science. Chemical philosophy has, in fact, now definitely entered on a new phase.

Looking back over the past, some indications of the coming change might have been perceived wholly unconnected, of course, with the recent experimental work which has served to ratify it. In a short paper entitled 'Speculative Ideas respecting the Constitution of Matter,' originally published in 1863, Graham conceived that the various kinds of matter, now recognised as different elementary substances, may possess one and the same ultimate or atomic molecule existing in different conditions of movement. This idea, in its essence, may be said to be as old as the time of Leucippus. To Graham as to Leucippus 'the action of the atom as one substance taking various forms by combinations unlimited, was enough to account for all the phenomena of the world. By separation and union with constant motion all things could be done.' But Graham developed the conception by independent thought, and in the light of experimentally ascertained knowledge which the world owes to his labours. He might have been cognisant of the speculations of the Greeks, but there is no evidence that he was knowingly influenced by them. In his paper Graham uses the terms atom and molecule if not exactly in the same sense that modern teaching demands, yet very different from that hitherto required by the limitations of contemporary chemical doctrine. He conceives of a lower order of atoms than the chemical atom of Dalton, and founds on his conception an explanation of chemical combination based upon a fixed combining measure, which he terms the *metron*, its relative weight being one for hydrogen, sixteen for oxygen, and so on with the other so-called 'elements.' Graham, in fact, like Davy before him, never committed himself to a belief in the indivisibility of the Daltonian atom. The original atom may, he thought, be far down.

The idea of a primordial ylé, or of the essential unity of matter, has persisted throughout the ages, and, in spite of much experimental work, some of it of the highest order, which was thought to have demolished it, it has survived, revivified and supported by analogies and arguments drawn from every field of natural inquiry. This idea of course was at the basis of the hypothesis of Prout, but which, even as modified by Dumas, was held to be refuted by the monumental work of Stas. But, as pointed out by Marignac and Dumas, anyone who will impartially look at the facts can hardly escape the feeling that there must be some reason for the frequent recurrence of atomic weights differing by so little

from the numbers required by the law which the work of Stas was supposed to disprove. The more exact study within recent years of the methods of determining atomic weights, the great improvement in experimental appliances and technique, combined with a more rigorous standard of accuracy demanded by a general recognition of the far-reaching importance of an exact knowledge of these physical constants, has resulted in intensifying the belief that some natural law must be at the basis of the fact that so many of the most carefully determined atomic weights on the oxygen standard are whole numbers. Nevertheless there were well authenticated exceptions which seemed to invalidate its universality. The proved fact that a so-called element may be a mixture of isotopes—substances of the same chemical attributes but of varying atomic weight—has thrown new light on the question. It is now recognised that the fractional values independently established in the case of any one element by the most accurate experimental work of various investigators are, in effect, 'statistical quantities' dependent upon a mixture of isotopes. This result, indeed, is a necessary corollary of modern conceptions of the inner mechanism of the atom. The theory that all elementary atoms are composed of helium atoms, or of helium and hydrogen atoms, may be regarded as an extension of Prout's hypothesis, with, however, this important distinction, that whereas Prout's hypothesis was at best a surmise, with little, and that little only weak, experimental evidence to support it, the new theory is directly deduced from well-established facts. The hydrogen isotope H_3, first detected by J. J. Thomson, of which the existence has been confirmed by Aston, would seem to be an integral part of atomic structure. Rutherford, by the disruption of oxygen and nitrogen has also isolated a substance of mass 3 which enters into the structure of atomic nuclei, but which he regards as an isotope of helium, which itself is built up of four hydrogen nuclei together with two cementing electrons. The atomic nuclei of elements of even atomic number would appear to be composed of helium nuclei only, or of helium nuclei with cementing electrons; whereas those of elements of odd atomic number are made up of helium and hydrogen nuclei together with cementing electrons. In the case of the lighter elements of the latter class the number of hydrogen nuclei associated with the helium nuclei is invariably three, except in that of nitrogen where it is two. The frequent occurrence of this group of three hydrogen nuclei indicates that it is structurally an isotope of hydrogen with an atomic weight of three and a nuclear charge of one. It is surmised that it is identical with the hypothetical 'nebulium' from which our 'elements' are held by astro-physicists to be originally produced in the stars through hydrogen and helium.

These results are of extraordinary interest as bearing on the question

of the essential unity of matter and the mode of genesis of the elements. Members of the British Association may recall the suggestive address on this subject of the late Sir William Crookes, delivered to the Chemical Section at the Birmingham meeting of 1886, in which he questioned whether there is absolute uniformity in the mass of the atoms of a chemical element, as postulated by Dalton. He thought, with Marignac and Schutzenberger, who had previously raised the same doubt, that it was not improbable that what we term an atomic weight merely represents a mean value around which the actual weights of the atoms vary within narrow limits, or, in other words, that the mean mass is 'a statistical constant of great stability.' No valid experimental evidence in support of this surmise was or could be offered at the time it was uttered. Maxwell pointed out that the phenomena of gaseous diffusion, as then ascertained, would seem to negative the supposition. If hydrogen, for example, were composed of atoms of varying mass it should be possible to separate the lighter from the heavier atoms by diffusion through a porous septum. 'As no chemist,' said Maxwell, 'has yet obtained specimens of hydrogen differing in this way from other specimens, we conclude that all the molecules of hydrogen are of sensibly the same mass, and not merely that their mean mass is a statistical constant of great stability.'[1] But against this it may be doubted whether any chemist had ever made experiments sufficiently precise to solve this point.

The work of Sir Norman Lockyer on the spectroscopic evidence for the dissociation of 'elementary' matter at transcendental temperatures, and the possible synthetic intro-stellar production of elements, through the helium of which he originally detected the existence, will also find its due place in the history of this new philosophy.

Sir J. J. Thomson was the first to afford direct evidence that the atoms of an element, if not exactly of the same mass, were at least approximately so, by his method of analysis of positive rays. By an extension of this method Mr. F. W. Aston has succeeded in showing that a number of elements are in reality mixtures of isotopes. It has been proved, for example, that neon, which has a mean atomic weight of about 20.2, consists of two isotopes having the atomic weights respectively of 20 and 22, mixed in the proportion of 90 per cent. of the former with 10 per cent. of the latter. By fractional diffusion through a porous septum an apparent difference of density of 0.7 per cent. between the lightest and heaviest fractions was obtained. The kind of experiment which Maxwell imagined proved the invariability of the hydrogen atom has sufficed to show the converse in the case of neon.

[1] Clerk-Maxwell, Art. 'Atom,' *Ency. Brit.* 9th Ed.

The element chlorine has had its atomic weight repeatedly determined, and, for special reasons, with the highest attainable accuracy. On the oxygen standard it is 35.46, and this value is accurate to the second decimal place. All attempts to prove that it is a whole number—35 or 36—have failed. When, however, the gas is analysed by the same method as that used in the case of neon it is found to consist of at least two isotopes of relative mass 35 and 37. There is no evidence whatever of an individual substance having the atomic weight 35.46. Hence chlorine is to be regarded as a complex element consisting of two principal isotopes of atomic weights 35 and 37 present in such proportion as to afford the mean mass 35.46. The atomic weight of chlorine has been so frequently determined by various observers and by various methods with practically identical results that it seems difficult to believe that it consists of isotopes present in definite and invariable proportion. Mr. Aston meets this objection by pointing out that all the accurate determinations have been made with chlorine derived originally from the same source, the sea, which has been perfectly mixed for æons. If samples of the element could be obtained from some other original source it is possible that other values of atomic weight would be obtained, exactly as in the case of lead in which the existence of isotopes in the metal found in various radioactive minerals was first conclusively established.

Argon, which has an atomic weight of 39.88, was found to consist mainly of an isotope having an atomic weight of 40, associated to the extent of about 3 per cent., with an isotope of atomic weight 36. Krypton and xenon are far more complex. The former would appear to consist of six isotopes, 78, 80, 82, 83, 84, 86; the latter of five isotopes, 129, 131, 132, 134, 136.

Fluorine is a simple element of atomic weight 19. Bromine consists of equal quantities of two isotopes, 79 and 81. Iodine, on the contrary, would appear to be a simple element of atomic weight 127. The case of tellurium is of special interest in view of its periodic relation to iodine, but the results of its examination up to the present are indefinite.

Boron and silicon are complex elements, each consisting of two isotopes, 10 and 11, and 28 and 29, respectively.

Sulphur, phosphorus, and arsenic are apparently simple elements. Their accepted atomic weights are practically integers.

All this work is so recent that there has been little opportunity, as yet, of extending it to any considerable number of the metallic elements. These, as will be obvious from the nature of the methods employed, present special difficulties. It is, however, highly probable that mercury is a mixed element consisting of many isotopes. These have been partially separated by Brönsted and Hervesy by fractional

distillation at very low pressures, and have been shown to vary very slightly in density. Lithium is found to consist of two isotopes, 6 and 7. Sodium is simple, potassium and rubidium are complex, each of the two latter elements consisting, apparently, of two isotopes. The accepted atomic weight of cæsium, 132.81, would indicate complexity, but the mass spectrum shows only one line at 133. Should this be confirmed cæsium would afford an excellent test case. The accepted value for the atomic weight is sufficiently far removed from a whole number to render further investigation desirable.

This imperfect summary of Mr. Aston's work is mainly based upon the account he recently gave to the Chemical Society. At the close of his lecture he pointed out the significance of the results in relation to the Periodic Law. It is clear that the order of the chemical or 'mean' atomic weights in the Periodic table has no practical significance; anomalous cases such as argon and potassium are simply due to the relative proportions of their heavier and lighter isotopes. This does not necessarily invalidate or even weaken the Periodic Law which still remains the expression of a great natural truth. That the expression as Mendeléeff left it is imperfect has long been recognised. The new light we have now gained has gone far to clear up much that was anomalous, especially Moseley's discovery that the real sequence is the atomic number, not the atomic weight. This is one more illustration of the fact that science advances by additions to its beliefs rather than by fundamental or revolutionary changes in them.

The bearing of the electronic theory of matter, too, on Prout's discarded hypothesis that the atoms of all elements were themselves built up of a primordial atom—his *protyle* which he regarded as probably identical with hydrogen—is too obvious to need pointing out. In a sense Prout's hypothesis may be said to be now re-established, but with this essential modification—the primordial atoms he imagined are complex and are of two kinds—atoms of positive and negative electricity—respectively known as protons and electrons. These, in Mr. Aston's words, are the standard bricks that Nature employs in her operations of element building.

The true value of any theory consists in its comprehensiveness and sufficiency. As applied to chemistry, this theory of 'the inner mechanism of the atom' must explain all its phenomena. We owe to Sir J. J. Thomson its extension to the explanation of the Periodic Law, the atomic number of an element, and of that varying power of chemical combination in an element we term valency. This explanation I give substantially in his own words. The number of electrons in an atom of the different elements has now been determined, and has been found to be equal to the atomic number of the element, that is to the position which the element occupies in the series when the elements are

arranged in the order of their atomic weights. We know now the nature and quantity of the materials of which the atoms are made up. The properties of the atom will depend not only upon these factors but also upon the way in which the electrons are arranged in the atom. This arrangement will depend on the forces between the electrons themselves and also on those between the electrons and the positive charges or protons. One arrangement which naturally suggested itself is that the positive charges should be at the centre with the negative electrons around it on the surface of a sphere. Mathematical investigation shows that this is a possible arrangement if the electrons on the sphere are not too crowded. The mutual repulsion of the electrons resents overcrowding, and Sir J. J. Thomson has shown that when there are more than a certain number of electrons on the sphere, the attraction of a positive charge, limited as in the case of the atom in magnitude to the sum of the charges on the electrons, is not able to keep the electrons in stable equilibrium on the sphere, the layer of electrons explodes and a new arrangement is formed. The number of electrons which can be accommodated on the outer layer will depend upon the law of force between the positive charge and the electrons. Sir J. J. Thomson has shown that this number will be eight with a law of force of a simple type.

To show the bearing of this result as affording an explanation of the Periodic Law, let us, to begin with, take the case of the atom of lithium, which is supposed to have one electron in the outer layer. As each element has one more free electron in its atom than its predecessor, glucinum, the element next in succession to lithium, will have two electrons in the outer layer of its atom, boron will have three, carbon four, nitrogen five, oxygen six, fluorine seven and neon eight. As there cannot be more than eight electrons in the outer layer, the additional electron in the atom of the next element, sodium, cannot find room in the same layer as the other electrons, but will go outside, and thus the atom of sodium, like that of lithium, will have one electron in its outer layer. The additional electron, in the atom of the next element, magnesium, will join this, and the atom of magnesium, like that of glucinum, will have two electrons in the outer layer. Again, aluminium, like boron, will have three; silicon, like carbon, four; phosphorus, like nitrogen, five; sulphur, like oxygen, six; chlorine, like fluorine, seven; and argon, like neon, eight. The sequence will then begin again. Thus the number of electrons, one, two, three, up to eight in the outer layer of the atom, will recur periodically as we proceed from one element to another in the order of their atomic weights, so that any property of an element which depends on the number of electrons in the outer layer of its atom will also recur periodically, which is precisely that remarkable property of the elements which is expressed

by the Periodic Law of Mendeléeff, or the Law of Octaves of Newlands.

The valency of the elements, like their periodicity, is a consequence of the principle that equilibrium becomes unstable when there are more than eight electrons in the outer layer of the atom. For on this view the chemical combination between two atoms, A and B, consists in the electrons of A getting linked up with those of B. Consider an atom like that of neon, which has already eight electrons in its outer layer; it cannot find room for any more, so that no atoms can be linked to it, and thus it cannot form any compounds. Now take an atom of fluorine, which has seven electrons in its outer layer; it can find room for one, but only one, electron, so that it can unite with one, but not with more than one, atom of an element like hydrogen, which has one electron in the outer layer. Fluorine, accordingly, is monovalent. The oxygen atom has six electrons; it has, therefore, room for two more, and so can link up with two atoms of hydrogen: hence oxygen is divalent. Similarly nitrogen, which has five electrons and three vacant places, will be trivalent, and so on. On this view an element should have two valencies, the sum of the two being equal to eight. Thus, to take oxygen as an example, it has only two vacant places, and so can only find room for the electrons of two atoms; it has, however, six electrons available for filling up the vacant places in other atoms, and as there is only one vacancy to be filled in a fluorine atom the electrons in an oxygen atom could fill up the vacancies in six fluorine atoms, and thereby attach these atoms to it. A fluoride of oxygen of this composition remains to be discovered, but its analogue, SF_6, first made known by Moissan, is a compound of this type. The existence of two valencies for an element is in accordance with views put forward some time ago by Abegg and Bödlander. Professor Lewis and Mr. Irving Longmuir have developed, with great ingenuity and success, the consequences which follow from the hypothesis that an octet of electrons surrounds the atoms in chemical compounds.

The term 'atomic weight' has thus acquired for the chemist an altogether new and much wider significance. It has long been recognised that it has a far deeper import than as a constant useful in chemical arithmetic. For the ordinary purposes of quantitative analysis, of technology, and of trade, these constants may be said to be now known with sufficient accuracy. But in view of their bearing on the great problem of the essential nature of matter and on the 'superlatively grand question, What is the inner mechanism of the atom?' they become of supreme importance. Their determination and study must now be approached from entirely new standpoints and by the conjoint action of chemists and physicists. The existence of isotopes has enormously widened the horizon. At first sight it would appear that we should

require to know as many atomic weights as there are isotopes, and the chemist may well be appalled at such a prospect. All sorts of difficulties start up to affright him, such as the present impossibility of isolating isotopes in a state of individuality, their possible instability, and the inability of his quantitative methods to establish accurately the relatively small differences to be anticipated. All this would seem to make for complexity. On the other hand, it may eventually tend towards simplification. If, with the aid of the physicist we can unravel the nature and configuration of the atom of any particular element, determine the number and relative arrangement of the constituent protons and electrons, it may be possible to arrive at the atomic weight by simple calculation, on the assumption that the integer rule is mathematically valid. This, however, is almost certainly not the case, owing to the influence of 'packing.' The little differences, in fact, may make all the difference. The case is analogous to that of the so-called gaseous laws in which the departures from their mathematical expression have been the means of elucidating the physical constitution of the gases and of throwing light upon such variations in their behaviour as have been observed to occur. There would appear, therefore, ample scope for the chemist in determining with the highest attainable accuracy the departures from the whole-number rule, since it is evident that much depends upon their exact extent.

These considerations have already engaged the attention of chemists. For some years past, a small International Committee, originally appointed in 1903, has made and published an annual report in which they have noted such determinations of atomic weight as have been made during the year preceding each report, and they have from time to time made suggestions for the amendment of the Tables of Atomic Weights, published in text-books and chemical journals, and in use in chemical laboratories. In view of recent developments, the time has now arrived when the work of this International Committee must be reorganised and its aims and functions extended. The mode in which this should be done has been discussed at the meeting in Brussels, in June last, of the International Union of Chemistry Pure and Applied, and has resulted in strengthening the constitution of the Committee and in a wide extension of its scope.

The crisis through which we have recently passed has had a profound effect upon the world. The spectacle of the most cultured and most highly developed peoples on this earth, armed with every offensive appliance which science and the inventive skill and ingenuity of men could suggest, in the throes of a death struggle must have made the angels weep. That dreadful harvest of death is past, but the aftermath remains. Some of it is evil, and the evil will persist for, it may be,

generations. There is, however, an element of good in it, and the good, we trust, will develop and increase with increase of years. The whole complexion of the world—material, social, economic, political, moral, spiritual—has been changed, in certain aspects immediately for the worse, in others prospectively for the better. It behoves us, then, as a nation to pay heed to the lessons of the war.

The theme is far too complicated to be treated adequately within the limits of such an address as this. But there are some aspects of it germane to the objects of this Association, and I venture, therefore, in the time that remains to me, to bring them to your notice.

The Great War differed from all previous internecine struggles in the extent to which organised science was invoked and systematically applied in its prosecution. In its later phases, indeed, success became largely a question as to which of the great contending parties could most rapidly and most effectively bring its resources to their aid. The chief protagonists had been in the forefront of scientific progress for centuries, and had an accumulated experience of the manifold applications of science in practically every department of human activity that could have any possible relation to the conduct of war. The military class in every country is probably the most conservative of all the professions and the slowest to depart from tradition. But when nations are at grips, and they realise that their very existence is threatened, every agency that may tend to cripple the adversary is apt to be resorted to—no matter how far it departs from the customs and conventions of war. This is more certain to be the case if the struggle is protracted. We have witnessed this fact in the course of the late War. Those who, realising that in the present imperfect stage of civilisation, wars are inevitable, and yet strove to minimise their horrors, and who formulated the Hague Convention of 1899, were well aware how these horrors might be enormously intensified by the applications of scientific knowledge, and especially of chemistry. Nothing shocked the conscience of the civilised world more than Germany's cynical disregard of the undertaking into which she had entered with other nations in regard, for instance, to the use of lethal gas in warfare. The nation that treacherously violated the Treaty of Belgium, and even applauded the action, might be expected to have no scruples in repudiating her obligations under the Hague Convention. April 25, 1915, which saw the clouds of the asphyxiating chlorine slowly wafted from the German trenches towards the lines of the Allies, witnessed one of the most bestial episodes in the history of the Great War. The world stood aghast at such a spectacle of barbarism. German *kultur* apparently had absolutely no ethical value. Poisoned weapons are employed by savages, and noxious gas had been used in Eastern warfare in early times, but its use was hitherto unknown among European nations.

How it originated among the Germans—whether by the direct unprompted action of the Higher Command, or, as is more probable, at the instance of persons connected with the great manufacturing concerns in Rhineland, has, so far as I know, not transpired. It was not so used in the earlier stages of the War, even when it had become a war of position. It is notorious that the great chemical manufacturing establishments of Germany had been, for years previously, sedulously linked up in the service of the war which Germany was deliberately planning—probably, in the first instance, mainly for the supply of munitions and medicaments. We may suppose that it was the tenacity of our troops, and the failure of repeated attempts to dislodge them by direct attack, that led to the employment of such foul methods. Be this as it may, these methods became part of the settled practice of our enemies, and during the three succeeding years, that is from April 1915 to September 1918, no fewer than eighteen different forms of poison—gases, liquids and solids—were employed by the Germans. On the principle of Vespasian's law, reprisals became inevitable, and for the greater part of three years we had the sorry spectacle of the leading nations of the world flinging the most deadly products at one another that chemical knowledge could suggest and technical skill contrive. Warfare, it would seem, has now definitely entered upon a new phase. The horrors which the Hague Convention saw were imminent, and from which they strove to protect humanity, are now, apparently, by the example and initiative of Germany, to become part of the established procedure of war. Civilisation protests against a step so retrograde. Surely comity among nations should be adequate to arrest it. If the League of Nations is vested with any real power, it should be possible for it to devise the means, and to ensure their successful application. The failure of the Hague Convention is no sufficient reason for despair. The moral sense of the civilised world is not so dulled but that, if roused, it can make its influence prevail. And steps should be taken without delay to make that influence supreme, and all the more so that there are agencies at work which would seek to perpetuate such methods as a recognised procedure of war. The case for what is called chemical warfare has not wanted for advocates. It is argued that poison gas is far less fatal and far less cruel than any other instrument of war. It has been stated that 'amongst the "mustard gas" casualties the deaths were less than 2 per cent., and when death did not ensue complete recovery generally ultimately resulted. . . . Other materials of chemical warfare in use at the Armistice do not kill at all; they produce casualties which, after six weeks in hospital, are discharged practically without permanent hurt.' It has been argued that, as a method of conducting war, poison-gas is more humane than preventive medicine. Preventive medicine has increased the unit dimension of an

army, free from epidemic and communicable disease, from 100,000 men to a million. 'Preventive medicine has made it possible to maintain 20,000,000 men under arms and abnormally free from disease, and so provided greater scope for the killing activities of the other military weapons. . . . Whilst the surprise effects of chemical warfare aroused anger as being contrary to military tradition, they were minute compared with those of preventive medicine. The former slew its thousands, whilst the latter slew its millions and is still reaping the harvest.' This argument carries no conviction. Poison gas is not merely contrary to European military tradition; it is repugnant to the right feeling of civilised humanity. It in no wise displaces or supplants existing instruments of war, but creates a new kind of weapon, of limitless power and deadliness. 'Mustard gas' may be a comparatively innocuous product as lethal substances go. It certainly was not intended to be such by our enemies. Nor, presumably, were the Allies any more considerate when they retaliated with it. Its effects, indeed, were sufficiently terrible to destroy the German *moral*. The knowledge that the Allies were preparing to employ it to an almost boundless extent was one of the factors that determined our enemies to sue for the Armistice. But if poisonous chemicals are henceforth to be regarded as a regular means of offence in warfare, is it at all likely that their use will be confined to 'mustard gas,' or indeed to any other of the various substances which were employed up to the date of the Armistice? To one who, after the peace, inquired in Germany concerning the German methods of making 'mustard gas,' the reply was:—'Why are you worrying about this when you know perfectly well that this is not the gas we shall use in the next war?'

I hold no brief for preventive medicine, which is well able to fight its own case. I would only say that it is the legitimate business of preventive medicine to preserve by all known means the health of any body of men, however large or small, committed to its care. It is not to its discredit if, by knowledge and skill, the numbers so maintained run into millions instead of being limited to thousands. On the other hand, 'an educated public opinion' will refuse to give credit to any body of scientific men who employ their talents in devising means to develop and perpetuate a mode of warfare which is abhorrent to the higher instincts of humanity.

This Association, I trust, will set its face against the continued degradation of science in thus augmenting the horrors of war. It could have no loftier task than to use its great influence in arresting a course which is the very negation of civilisation.

PROBLEMS OF PHYSICS.

ADDRESS BY

PROFESSOR O. W. RICHARDSON, D.Sc., F.R.S.,

PRESIDENT OF THE SECTION.

My predecessor in office a year ago reminded you that the theoretical researches of Einstein and Weyl suggest that not merely the material universe but space itself is perhaps finite. As to the probabilities I do not wish to express an opinion; but the statement is significant of the extent of the revolution in the conceptions and fundamental principles of physics now in progress. That space need not be infinite has, I believe, long been recognised by geometricians, and appropriate geometries to meet its possible limitations have been devised by ingenious mathematicians. I doubt, however, whether these inventive gentlemen ever dreamed that their schemes held any objective validity such as would assist the astronomer and the physicist in understanding and classifying material phenomena. It is not certain that they will; but the possibility is definite. Apart from this, the whole development of relativity is an extraordinary triumph for pure mathematics. Had Einstein not found his entire calculus ready to hand, owing to the purely mathematical work of Christoffel, Riemann, and others, it seems certain that the development of generalised relativity would have been much slower. It is a pleasure to be able to acknowledge this indebtedness of physics and astronomy to pure mathematics.

Relativity is the revolutionary movement in physics which has caught the public eye, perhaps because it deals with familiar conceptions in a manner which for the most part is found pleasantly incomprehensible. But it is only one of a number of revolutionary changes of comparable magnitude. Among these we have to place the advent of the quantum, the significance of which I hope we shall thoroughly discuss early next week. The various consequences of the electronic structure of matter are still unfolding themselves to us, and are increasing our insight into the most varied phenomena at a rate which must have appeared incredible only a few decades ago.

The enormous and far-reaching importance of the discoveries being made at Cambridge by Sir Ernest Rutherford cannot be over-emphasised. These epoch-making discoveries relate to the structure and properties of the nuclei of atoms. At the present time we have, I think, to accept it as a fact that the atoms consist of a positively charged nucleus of minute size, surrounded at a fairly respectful distance by the number of electrons requisite to maintain the structure electrically neutral. The nucleus contains all but about one-two-thousandth part of the mass of the atom, and its electric charge is numerically equal to that of the negative electron multiplied by what is called the atomic number of the atom, the atomic number being the number

which is obtained when the chemical elements are enumerated in the order of the atomic weights; thus hydrogen=1, helium=2, lithium=3, and so on. Consequently the number of external electrons in the atom is also equal to the atomic number. The evidence, derived from many distinct and dissimilar lines of inquiry, which makes it necessary to accept the foregoing statements as facts, will be familiar to members of this Section of the British Association, which has continually been in the forefront of contemporary advances in physical science. But I would remind you in passing that one of the important pieces of evidence was supplied by Professor Barkla's researches on the scattering of X-rays by light atoms.

The diameters of the nuclei of the atoms are comparable with one-millionth of one-millionth part of a centimetre, and the problem of finding what lies within the interior of such a structure seems at first sight almost hopeless. It is to this problem which Rutherford has addressed himself by the direct method of bombarding the nuclei of the different atoms with the equally minute high-velocity helium nuclei (alpha-particles) given off by radioactive substances, and examining the tracks of any other particles which may be generated as a result of the impact. A careful and critical examination of the results shows that hydrogen nuclei are thus expelled from the nuclei of a number of atoms such as nitrogen and phosphorus. On the other hand, oxygen and carbon do not eject hydrogen under these circumstances, although there is evidence in the case of oxygen and nitrogen of the expulsion of other sub-nuclei whose precise structure is a matter for further inquiry.

The artificial transmutation of the chemical elements is thus an established fact. The natural transmutation has, of course, been familiar for some years to students of radioactivity. The philosopher's stone, one of the alleged chimeras of the mediæval alchemists, is thus within our reach. But this is only part of the story. It appears that in some cases the kinetic energy of the ejected fragments is greater than that of the bombarding particles. This means that these bombardments are able to release the energy which is stored in the nuclei of atoms. Now, we know from the amount of heat liberated in radioactive disintegration that the amount of energy stored in the nuclei is of a higher order of magnitude altogether, some millions of times greater, in fact, than that generated by any chemical reaction such as the combustion of coal. In this comparison, of course, it is the amount of energy per unit mass of reacting or disintegrating matter which is under consideration. The amounts of energy which have thus far been released by artificial disintegration of the nuclei are in themselves small, but they are enormous in comparison with the minute amounts of matter affected. If these effects can be sufficiently intensified there appear to be two possibilities. Either they will prove uncontrollable, which would presumably spell the end of all things,[1]

[1] To reassure the nervous I would, however, interpolate the comforting thought that this planet has held considerable quantities of radioactive matter for a very long time without anything very serious happening so far as we know.

or they will not. If they can be both intensified and controlled then we shall have at our disposal an almost illimitable supply of power which will entirely transcend anything hitherto known. It is too early yet to say whether the necessary conditions are capable of being realised in practice, but I see no elements in the problem which would justify us in denying the possibility of this. It may be that we are at the beginning of a new age, which will be referred to as the age of sub-atomic power. We cannot say; time alone will tell.

Thermionic Emission.

With your permission, I will now descend a little way from the summit of Mount Olympus, and devote the rest of my address to a sober review of the present state of some of the questions with which my own thoughts have been more particularly occupied. At the Manchester meeting of the Association in 1915 I had the privilege of opening a discussion on thermionic emission—that is to say, the emission of electrons and ions by incandescent bodies. I recall that the opinion was expressed by some of the speakers that these phenomena had a chemical origin. That view, I venture to think, is one which would find very few supporters now. It is not that any new body of fact has arisen in the meantime. The important facts were all established before that time, but they were insufficiently appreciated, and their decisiveness was inadequately realised.

It may be worth while to revert for a moment to the issues in that controversy, already moribund in 1915, because it has been closely paralleled by similar controversies relating to two other groups of phenomena—namely, photoelectric emission and contact electromotive force—which, as we shall see, are intimately connected with thermionic emission. The issue was not as to whether thermionic emission may be looked upon simply as a type of chemical reaction. Such an issue would have been largely a matter of nomenclature. Thermionic electron emission has many features in common with a typical reversible chemical reaction such as the dissociation of calcium carbonate into lime and carbon dioxide. There is a good deal to be said for the point of view which regards thermionic emission as an example of the simplest kind of reversible chemical action, namely, that kind which consists in the dissociation of a neutral atom into a positive residue and a negative electron, inasmuch as we know that the negative electron is one of the really fundamental elements out of which matter is built up. The issue in debate was, however, of a different character. It was suggested that the phenomenon was not primarily an emission of electrons from the metallic or other source, but was a secondary phenomenon, a kind of by-product of an action which was primarily a chemical reaction between the source of electrons and some other material substance such as the highly attenuated gaseous atmosphere which surrounded it. This suggestion carried with it either implicitly or explicitly the view that the source of power behind the emission was not the thermal energy of the source, but was the chemical energy of the postulated reactions.

This type of view has never had any success in elucidating the

phenomena, and I do not feel it necessary at this date to weary you with a recital of the facts which run entirely counter to it, and, in fact, definitely exclude it as a possibility. They have been set forth at length elsewhere on more than one occasion. I shall take it to be established that the phenomenon is physical in its origin and reversible in its operation.

Establishing the primary character of the phenomenon does not, however, determine its nature or its immediate cause. Originally I regarded it as simply kinetic, a manifestation of the fact that as the temperature rose the kinetic energy of some of the electrons would begin to exceed the work of the forces by which they are attracted to the parent substance. With this statement there is, I think, no room for anyone to quarrel, but it is permissible to inquire how the escaping electrons obtain the necessary energy. One answer is that the electrons have it already in the interior of the substance by virtue of their energy of thermal agitation. But thermal agitations now appear less simple than they used to be regarded, and in any event they do not exhaust the possibilities.

We know that when light of short enough wave-length falls on matter it causes the ejection of electrons from it—the so-called photoelectric effect. Since the formula for the radiation emitted by a body at any given temperature contains every wave-length without limitation, there must be some emission of electrons from an incandescent body as the result of the photoelectric effect of its own luminosity. Two questions obviously put themselves. Will this photoelectric emission caused by the whole spectrum of the hot body vary as the temperature of the incandescent body is raised in the way which is known to characterise thermionic emission? A straightforward thermodynamic calculation shows that this is to be expected from the theoretical standpoint, and the anticipation has been confirmed by the experiments of Professor W. Wilson. Thus the autophotoelectric emission has the correct behaviour to account for the thermionic emission. The other question is: Is it large enough? This is a question of fact. I have considered the data very carefully. There is a little uncertainty in some of the items, but when every allowance is made there seems no escape from the conclusion that the photoelectric effect of the whole spectrum is far too small to account for thermionic emission.

This question is an important one, apart from the particular case of thermionic emission. The same dilemma is met with when we seek for the actual *modus operandi* of evaporation, chemical action, and a number of other phenomena. These, so far as we know, might be fundamentally either kinetic or photochemical or a mixture of both. In my judgment the last alternative is the most probable. (I am using the term photochemical here in the wide sense of an effect of light in changing the composition of matter, whether the parts affected are atoms, groups of atoms, ions, or electrons.) For example, the approximation about boiling points known as Trouton's rule is a fairly obvious deduction from the photochemical standpoint. The photochemical point of view has recently been put very strongly by Perrin,

who would make it the entire *motif* of all chemical reaction, as well as of radioactivity and changes of state. In view of the rather minor part it seems to play in thermionic emission, where one would *a priori* have expected light to be especially effective, this is probably claiming too much for it, but the chemical evidence contains one item which is certainly difficult to comprehend from the kinetic standpoint. The speed of chemical decomposition of certain gases is independent of their volume, showing that the decomposition is not due to molecular collisions. The speed does, however, increase very rapidly with rising temperature. What the increased temperature can do except increase the number and intensity of the collisions, factors which the independence of volume at constant temperature show to be without effect, and increase the amount of radiation received by the molecules, is not too obvious. It seems, however, that, according to calculations by Langmuir,[2] the radiation theory does not get us out of this difficulty; for, just as in the ordinary photoelectric case, there is nothing like enough radiation to account for the observed effects. It seems that in the case of these mono-molecular reactions the phenomena cannot be accounted for either by simple collisions, or by radiation, or by a mixture of both, and it is necessary to fall back on the internal structure of the decomposing molecule. This is complex enough to afford material sufficient to cover the possibilities; but, from the standpoint of the temperature energy relations of its parts, it cannot at present be regarded as much more than a field for speculation.

Contact Electricity.

A controversy about the nature of the contact potential difference between two metals, similar to that to which I have referred in connection with thermionic emission, has existed for over a century. In 1792 Volta wrote: 'The metals . . . can by themselves, and of their own proper virtue, excite and dislodge the electric fluid from its state of rest.' The contrary position that the electrical manifestations are inseparably connected with chemical action was developed a few years later by Fabroni. Since that time electrical investigators have been fairly evenly divided between these two opposing camps. Among the supporters of the intrinsic or contact view of the type of Volta we may recall Davy, Helmholtz, and Kelvin. On the other side we have to place Maxwell, Lodge, and Ostwald. In 1862 we find Lord Kelvin[3] writing: 'For nearly two years I have felt quite sure that the proper explanation of voltaic action in the common voltaic arrangement is very near Volta's, which fell into discredit because Volta or his followers neglected the principle of the conservation of force.' On the other hand, in 1896 we find Ostwald[4] referring to Volta's views as the origin of the most far-reaching error in electrochemistry, which the greatest part of the scientific work in that domain has been occupied in fighting almost ever since. These are cited merely as representative specimens of the opinions of the protagonists.

[2] *Journ. Am. Chem. Soc.*, vol. xlii., p. 2190 (1920).
[3] Papers on Electrostatics and Magnetism, p. 318.
[4] *Elektochemie, Ihre Geschichte und Lehre*, p. 65, Leipzig (1896).

Now, there is a close connection between thermionic emission and contact potential difference, and I believe that a study of thermionic emission is going to settle this little dispute. In fact, I rather think it has already settled it, but before going into that matter I would like to explain how it is that there is a connection between thermionic emission and contact potential difference, and what the nature of that connection is.

Imagine a vacuous enclosure, either impervious to heat or maintained at a constant temperature. Let the enclosure contain two different electron-emitting bodies, A and B. Let one of these, say A, have the power of emitting electrons faster than the other, B. Since they are each receiving as well as emitting electrons, A will acquire a positive and B a negative charge under these circumstances. Owing to these opposite charges A and B will now attract each other, and useful work can be obtained by letting them come in contact. After the charges on A and B have been discharged by bringing them in contact, let the bodies be quickly separated and moved to their original positions. This need involve no expenditure of work, as the charges arising from the electron emission will not have had time to develop. After the charges have had time to develop the bodies can again be permitted to move together under their mutual attraction, and so the cycle can be continued an indefinite number of times. In this way we have succeeded in imagining a device which will convert all the heat energy from a source at a uniform temperature into useful work.

Now, the existence of such a device would contravene the second law of thermodynamics. We are therefore compelled either to deny the principles of thermodynamics or to admit that there is some fallacy as to the pretended facts in the foregoing argument. We do not need to hesitate between these alternatives, and we need only look to see how the alleged behaviour of A and B will need to be modified in order that no useful work may appear. There are two alternatives. Either A and B necessarily emit equal numbers (which may include the particular value zero) of electrons at all temperatures, or the charges which develop owing to the unequal rate of emission are not discharged, even to the slightest degree, when the two bodies are placed in contact.

The first alternative is definitely excluded by the experimental evidence, so I shall proceed to interpret the second. It means that bodies have natural states of electrification whereby they become charged to definite potential differences whose magnitudes are independent of their relative positions. There is an intrinsic potential difference between A and B which is the same, at a given temperature, whether they are at a distance apart or in contact. In the words of Volta, which I have already quoted, 'the metals can by themselves, and of their own proper virtue, excite and dislodge the electric fluid from its state of rest.'

Admitting that the intrinsic potentials exist, a straightforward calculation shows that they are intimately connected with the magnitudes of the thermionic emission at a given temperature. The relation is, in fact, governed by the following equation: If A and B denote the saturation thermionic currents per unit area of the bodies A and

B respectively, and V is the contact potential difference between them at the absolute temperature T, then $V=\frac{kT}{e} \log \frac{A}{B}$ where k is the gas constant calculated for a single molecule (Boltzmann's constant), and e is the electronic charge.

I have recently, with the help of Mr. F. S. Robertson, obtained a good deal of new information on this question from the experimental side. We have made measurements of the contact potential difference between heated filaments and a surrounding metallic cylinder, both under the high-vacuum and gas-free conditions which are now attainable in such apparatus, and also when small known pressures of pure hydrogen are present. As is well known, both contact potentials and thermionic emission are very susceptible to minute traces of gas, but we find that under the best conditions as to freedom from gas there is a contact potential of the order of one volt between a pure tungsten filament and a thoriated filament. We have also been able to measure the thermionic emissions from the filaments at the same time, and we find that the contact potential calculated from them with the help of the foregoing equation is within 20 per cent. of the measured value. Considering the experimental difficulties, this is a very substantial agreement. Whilst the evidence is not yet as complete as I hope to make it, it goes a long way towards disproving the chemical view of the origin of contact potential difference.

From what has been said you will realise that the connection between contact potentials and thermionic emissions is a very close one. I would, however, like to spend a moment in developing it from another angle. To account for the facts of thermionic emission it is necessary to assume that the potential energy of an electron in the space just outside the emitter is greater than that inside by a definite amount, which we may call w. The existence of this w, which measures the work done when an electron escapes from the emitter, is required by the electron-atomic structure of matter and of electricity. Its value can be deduced from the temperature variation of thermionic emission, and, more directly, from the latent heats absorbed or generated when electrons flow out of or into matter. These three methods give values of w which, allowing for the somewhat considerable experimental difficulties, are in fair agreement for any particular emitter. The data also show that in general different substances have different values of w. This being so, it is clear that when uncharged bodies are placed in contact the potential energies of the electrons in one will in general be different from those of the electrons in the other. If, as in the case of the metals, the electrons are able to move freely they will so move until an electric field is set up which equilibrates this difference of potential energy. There will thus be an intrinsic or contact difference of potential between metals which is equivalent to the difference in the values of w and is equal to the difference in w divided by the electronic charge.[5]

[5] This statement is only approximately true. In order to condense the argument certain small effects connected with the Peltier effect at the junction between the metals have been left out of consideration.

Photoelectric Action.

We have seen that there is a connection on broad lines between thermionic emission and both contact potentials on the one hand and photoelectric emission on the other. The three groups of phenomena are also related in detail and to an extent which up to the present has not been completely explored. In order to understand the present position, let us review briefly some of the laws of photoelectric action as they have revealed themselves by experiments on the electrons emitted from metals when illuminated by visible and ultra-violet light.

Perhaps the most striking feature of photoelectric action is the existence of what has been called the threshold frequency. For each metal whose surface is in a definite state there is a definite frequency n_0, which may be said to determine the entire photoelectric behaviour of the metal. The basic property of the threshold frequency n_0 is this: When the metal is illuminated by light of frequency less than n_0 no electrons are emitted, no matter how intense the light may be. On the other hand, illumination by the most feeble light of frequency greater than n_0 causes some emission. The frequency n_0 signalises a sharp and absolute discontinuity in the phenomena.

Now let us inquire as to the kinetic energy of the electrons which are emitted by a metal when illuminated by monochromatic light of frequency, let us say, n. Owing to the fact that the emitted electrons may originate from different depths in the metal, and may undergo collisions at irregular intervals, it is only the maximum kinetic energy of those which escape which we should expect to exhibit simple properties. As a matter of fact, it is found that the maximum kinetic energy is equal to the difference between the actual frequency n and the threshold frequency n_0 multiplied by Planck's constant h. In mathematical symbols, if v is the velocity of the fastest emitted electron, m its mass, e its charge, and V the opposing potential required to bring it to rest,

$$eV = \tfrac{1}{2}\, m\, v^2 = h\ (n - n_0).$$

From this equation we see that the threshold frequency has another property. It is evidently that frequency for which kinetic energy and stopping potential fall to zero. This suggests strongly, I think, that the reason the electron emission ceases at n_0 is that the electrons are not able to get enough energy from the light to escape from the metal, and not that they are unable to get any energy from the light.

The threshold frequencies have another simple property. If we measure the threshold frequencies for any pair of metals, and at the same time we measure the contact difference of potential K between them, we find that K is equal to the difference between their threshold frequencies multiplied by this same constant h divided by the electronic charge e.

These results, as well as others which I have not time to enumerate, admit of a very simple interpretation if we assume that when illuminated by light of frequency n the electrons individually acquire an amount of energy hn. We have seen that in order to account for thermionic phenomena it is necessary to assume that the electrons

have to do a certain amount of work w to get away from the emitter. There is no reason to suppose that photoelectrically emitted electrons can avoid this necessity. Let us suppose that this work is also definite for the photoelectric electrons and let us denote its value by hn_0. Then no electron will be able to escape from the metal until it is able to acquire an amount of energy at least equal to hn_0 from the light—that is to say, under the suppositions made—until n becomes at least as great as n_0. Thus n_0 will be identical with the frequency which we have called the threshold frequency, and the maximum energy of any electron after escaping will be $h\ (n-n_0)$.

The relation between threshold frequencies and contact potential difference raises another issue. We have seen that the contact potential difference between two metals must be very nearly equal to the difference between the amounts of work w for the electrons to get away from the two metals by thermionic action, divided by the electronic charge e. The photoelectric experiments show that the contact electromotive force is also nearly equal to the differences of the threshold frequencies multiplied by h/e. It follows that the photoelectric work hn_0 must be equal to the thermionic work w to the same degree of accuracy. We have to except here a possible constant difference between the two. I do not see, however, how any value other than zero for such a constant could be given a rational interpretation, as it would have to be the same for all substances and frequencies. The photoelectric and thermionic works are known to agree to within about one volt. To decide how far they are identical needs better experimental evidence than we have at present. The indirect evidence for their substantial identity (that is to say, within the limits of accuracy referred to above) is stronger at the moment than the direct evidence.

I do not think that the complete identity of the thermionic work w and the photoelectric hn_0 is a matter which can be inferred *a priori*. What we should expect depends to a considerable extent on the condition of the electrons in the interior of metals. We cannot pretend to any real knowledge of this at present; the various current theories are mere guesswork. Unless the electrons which escape all have the same energy when inside the metal we should expect the thermionic value to be an average taken over those which get out. The photoelectric value, on the other hand, should be the minimum pertaining to those internal electrons which have most energy. The apparent sharpness of the threshold frequency is also surprising from some points of view. There seems to be scope for a fuller experimental examination of these questions.

I have spoken of the threshold frequency as though it were a perfectly definite quantity. No doubt it is when the condition of the body is or can be definitely specified, but it is extraordinarily sensitive to minute changes in the conditions of the surface, such as may be caused, for example, by the presence of extremely attenuated films of foreign matter. For this reason we should accept with a certain degree of reserve statements which appear from time to time that photoelectric action is some parasitic phenomenon, inasmuch as it can be made to disappear by improvement of vacuum or other change in the conditions.

What has generally happened in these investigations is that something has been done to the illuminated surface which has raised its threshold frequency above that of the shortest wave-length in the light employed in the test. Unless they are accompanied by specific information about the changes which have taken place in the threshold frequency, such statements are of little value at the present stage of development of this subject.

Interesting calculations have been made by Frenkel which bring surface tension into close connection with the thermionic work w. Broadly speaking, there can be little doubt that a connection of this nature exists, but whether the relation is as simple as that given by the calculations is open to doubt. It should be possible to answer this question definitely when we have more precise information about the disposition of the electrons in atoms such as the continuous progress in X-ray investigation seems to promise.

Light and X-Rays.

One of the great achievements of experimental physics in recent years has been the demonstration of the essential unity of X-rays and ordinary light. X-rays have been shown to be merely light of particularly high frequency or short wave-length, the distinction between the two being one of degree rather than of kind. The foundations of our knowledge of X-ray phenomena were laid by Barkla, but the discovery and development of the crystal diffraction methods by v. Laue, the Braggs, Moseley, Duane, and de Broglie have established their relations with ordinary light so clearly that he who runs may read their substantial identity. The actual gap in the spectrum of the known radiations between light and X-rays is also rapidly disappearing. The longest stride into the region beyond the ultra-violet was made by Lyman with the vacuum grating spectroscope which he developed. For a short time Professor Bazzoni and I held the record in this direction with our determination of the short wave limit of the helium spectrum, which is in the neighbourhood of 450 Ångstrom units. More recently this has been passed by Millikan, who has mapped a number of lines extending to about 200 Ångstrom units—that is to say, more than four octaves above the violet limit of the visible spectrum. I am not sure what is the longest X-ray which has been measured, but I find a record of a Zinc L-ray by Friman[6] of a wave-length of 12.346 Ångstrom units. There is thus at most a matter of about four octaves still to be explored. In approaching this unknown region from the violet end the most characteristic property of the radiations appears to be their intense absorption by practically every kind of matter. This result is not very surprising from the quantum standpoint. The quantum of these radiations is in excess of that which corresponds to the ionising potential of every known molecule, but it is of the same order of magnitude. Furthermore, it is large enough to reach not only the most superficial, but also a number of the deeper-seated

[6] *Phil. Mag.*, vol. xxxii., p. 494 (1916).

electrons of the atoms. There is evidence, both theoretical and experimental, that the photoelectric absorption of radiation is most intense when its quantum exceeds the minimum quantum necessary to eject the absorbing electron but does not exceed it too much. In the simplest theoretical case the absorption is zero for radiations whose frequencies lie below the minimum quantum, rises to a maximum for a frequency comparable with the minimum, and falls off to zero again at infinite frequency. This case has not been realised in practice, but, broadly judged, the experimental data are in harmony with it. On these general grounds we should expect intense absorption by all kinds of matter for the radiation between the ultra-violet and the X-ray region.

The closeness of the similarity in the properties of X-rays and light is, I think, even yet inadequately realised. It is not merely a similarity along broad lines, but it extends to a remarkable degree of detail. It is perhaps most conspicuous in the domains of photoelectric action and of the inverse phenomenon of the excitation of radiation or spectral lines by electron impacts. Whilst there may still be room for doubt as to the precise interpretation of some of the experimental data, the impression I have formed is that each important advance tends to unify rather than to disintegrate these two important groups of phenomena.

SECTION B.—CHEMISTRY.

THE LABORATORY ORGANI

ADDRESS BY

M. O. FORSTER, D.Sc., F.R.S.,

PRESIDENT OF THE SECTION.

Many and various are the reasons which have been urged, at different periods of its history, for stimulating the study of chemistry. In recent years these have been either defensive or frankly utilitarian, in the latter feature recalling the less philosophic aspects of alchemy; moreover, it is to be feared that a substantial proportion of those who have lately hastened to prepare themselves for a chemical career have been actuated by this inducement. It is the duty, therefore, of those who speak with any degree of experience to declare that the only motive for pursuing chemistry which promises anything but profound disappointment is an affection for the subject sufficiently absorbing to displace the attraction of other pursuits. Even to the young chemist who embarks under this inspiration the prospect of success as recognised by the world is indeed slender, but, as his knowledge grows and the consequent appreciation of our ignorance widens, enthusiasm for the beauty and mystery of surrounding nature go far in compensating for the disadvantages of his position. On the other hand, he who has been beguiled into embracing chemistry on the sole ground of believing it to be a 'good thing' will either desert it expeditiously or almost surely starve and shower purple curses upon his advisers.

In one respect chemistry resembles measles—every boy and girl should have it, lest an attack in later life should prove more serious. Moreover, whilst it is not only unnecessary, but most undesirable, to present the subject as if every boy and girl were going to be a chemist, it is most important to present it in such a manner that every educated citizen may realise the intimate part which chemistry plays in his daily life. Not only do chemical principles underlie the operations of every industry, but every human being—indeed, every living plant and animal—is, during each moment of healthy life, a practical organic and physical chemist, conducting analytical and synthetical processes of the most complex order with imperturbable serenity. No other branch of knowledge can appeal for attention on comparable grounds; and without suggesting that we should all, individually, acquire sufficient chemical understanding fully to apprehend the changes which our bodies effect so punctually and so precisely—for this remains beyond the power of trained chemists—it may be claimed that an acquaintance with the general outlines of chemistry would add to the mental equipment of our people a source of abundant intellectual pleasure which is now unfairly denied them. We have been told that the world shall be made a fit place for heroes to live in; but is not the preliminary

to this ideal an exposition to those heroes of the wonder and beauty of the world which they already occupy, on the principle that if you cannot have what you like, it is elementary wisdom to like what you have? In following the customary practice of surveying matters of interest which have risen from our recent studies, therefore, it is the purpose of this address to emphasise also those æsthetic aspects of chemistry which offer ample justification for the labour which its pursuit involves.

What is breakfast to the average man? A hurried compromise between hunger and the newspaper. How does the chemist regard it? As a daily miracle which gains, rather than loses, freshness as the years proceed. For just think what happens. Before we reach the table frizzled bacon, contemplated or smelt, has actuated a wonderful chemical process in our bodies. The work of Pavlov has shown that if the dog has been accustomed to feed from a familiar bowl the sight of that bowl, even empty, liberates from the appropriate glands a saliva having the same chemical composition as that produced by snuffing the food. This mouth-watering process, an early experience of childhood, is known to the polite physiologist as a 'psychic reflex,' and the various forms assumed by psychic reflex, responding to the various excitations which arise in the daily life of a human being, must be regarded by the chemical philosopher as a series of demonstrations akin to those which he makes in the laboratory, but hopelessly inimitable with his present mental and material resources. For, extending this principle to the other chemical substances poured successively into the digestive tract, we have to recognise that the minute cells of which our bodies are co-ordinated assemblages possess and exercise a power of synthetic achievement contrasted with which the classical syntheses, occasionally enticing the modern organic chemist to outbursts of pride, are little more than hesitating preliminaries. Such products of the laboratory, elegant as they appear to us, represent only the fringe of this vast and absorbing subject. Carbohydrates, alkaloids, glucosides and purines, complex as they seem when viewed from the plane of their constituent elements, are but the molecular debris strewing the path of enzyme action and photochemical synthesis, whilst the enzymes produced in the cells, and applied by them in their ceaseless metamorphoses, are so far from having been synthesised by the chemist as to have not even yet been isolated in purified form, although their specific actions may be studied in the tissue-extracts containing them.

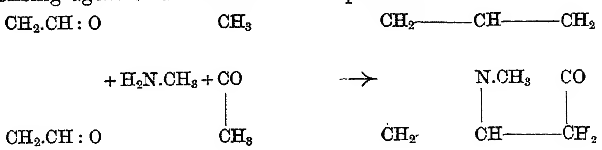
Reflect for a moment on the specific actions. The starch in our toast and porridge, the fat in our butter, the proteins in our bacon, all insoluble in water, by transformations otherwise unattainable in the laboratory are smoothly and rapidly rendered transmissible to the blood, which accepts the products of their disintegration with military precision. Even more amazing are the consequences. Remarkable as the foregoing analyses must appear, we can dimly follow their progress by comparison with those more violent disruptions of similar materials revealed to us by laboratory practice, enabling such masters of our craft as Emil Fischer to isolate the resultant individuals. Concurrently with such analyses, however, there proceed syntheses which we can scarcely

visualise, much less imitate. The perpetual elaboration of fatty acids from carbohydrates, of proteins from amino-acids, of zymogens and hormones as practised by the living body are beyond the present comprehension of the biochemist; but their recognition is his delight, and the hope of ultimately realising such marvels provides the dazzling goal towards which his efforts are directed.

The Vegetable Alkaloids.

The joyous contemplation of these wonders is an inalienable reward of chemical study, but it is denied to the vast majority of our people. The movements of currency exchange, to which the attention of the public has been directed continuously for several years, are clumsy contortions compared with the chemical transformations arising from food exchange. It should not be impossible to bring the skeleton of these transformations within the mental horizon of those who take pleasure in study and reflection; and to those also the distinction between plants and animals should be at least intelligible. The wonderful power which plants exercise in building up their tissues from carbonic acid, water and nitrogen, contrasted with the powerlessness of animals to utilise these building materials until they have been already assembled by plants, is a phenomenon too fundamental and illuminating to be withheld, as it now is, from all but the few. For by its operation the delicate green carpet, which we all delight in following through the annual process of covering the fields with golden corn, is accomplishing throughout the summer months a vast chemical synthesis of starch for our benefit. Through the tiny pores in those tender blades are circulating freely the gases of the atmosphere, and from those gases—light, intangible nothingness, as we are prone to regard them—this very tangible and important white solid compound is being elaborated. The chemist cannot do this. Plants accomplish it by their most conspicuous feature, greenness, which enables them to put solar energy into cold storage; they are accumulating fuel for subsequent development of bodily heat energy. Side by side with starch, however, these unadvertised silent chemical agencies elaborate molecules even more imposing, in which nitrogen is interwoven with the elements of starch, and thus are produced the vegetable alkaloids.

In this province the chemist has been more fortunate, and successive generations of students have been instructed in the synthesis of piperine, coniine, trigonelline, nicotine, and extensions from the artificial production of tropine; but until quite recently his methods have been hopelessly divergent from those of the plant. Enlightening insight into these, however, was given just four years ago by R. Robinson, who effected a remarkably simple synthesis of tropinone by the mere association of succindialdehyde, methylamine, and acetone in water, unassisted by a condensing agent or an increase of temperature:



Although the yield was very small, it reached 42 per cent. when acetone was replaced by a salt of its dicarboxylic acid, which might easily arise from citric acid as one of the intermediate compounds used by plants in their synthetical exercises.

Based upon this experiment, R. Robinson (1917) has developed an attractive explanation of the phytochemical synthesis of alkaloids, in which the genesis of a pyrrolidine, piperidine, quinunuclidine, or *iso*-quinoline group is shown to be capable of proceeding from the association and interaction of an amino-acid, formaldehyde, acetonedicarboxylic acid and the intermediate products of these, taking place under the influence of oxidation, reduction, and condensation such as the plant is known to effect. It would scarcely be fair to the resourceful skill embodied in this theory to attempt an abbreviated description of the methods by which molecules as complex even as those of morphine and narcotine may be developed. Ornithine ($\alpha\delta$-diaminovaleric acid) is represented as the basis of hygrine, cuschygrine, and the tropine alkaloids, whilst the coniine group may spring from lysine ($\alpha\epsilon$-diaminocaproic acid). A particularly interesting application of these principles has been made with reference to the vital synthesis of harmine, which W. H. Perkin and R. Robinson (1919) represent as arising from a hydroxytryptophan as yet undiscovered; meanwhile they have shown that harman is identical with the base obtained by Hopkins and Cole on oxidising tryptophan itself with ferric chloride. Thus it may be claimed that Robinson's theory represents a notable advance in our conception of these vital changes, and that by means of the carbinolamine and aldol condensations involved fruitful inquiries into constitution and the mechanism of synthesis will follow.

The Nucleic Acids.

Owing to the venerable position occupied by alkaloids in the systematic development of chemical science, and to the success which has attended elucidation of their structure, many of us have become callous to the perpetual mystery of their elaboration. Those who seek fresh wonders, however, need only turn to the nucleic acids in order to satisfy their curiosity. For in the nucleic acid of yeast the chemist finds a definite entity forming a landmark in the path of metabolic procedure, a connecting link between the undefined molecules of living protein and the crystallisable products of katabolic disintegration.

Let us review this remarkable substance. With an empirical formula, $C_{38}H_{49}O_{29}N_{15}P_4$, it has a molecular weight (1303) exceeding that of the octadecapeptide (1213) synthesised by Fischer (1907), although considerably below those of the penta-(penta-acetyl-*m*-digalloyl)-β-glucose (2136) produced by Fischer and Bergmann (1918), and of the hepta-(tribenzoylgalloyl)-*p*-iodophenylmaltosazone (4021) elaborated by Fischer and Freudenberg (1912). Nevertheless, its intrinsic importance is transcendent. In the language of chemistry it is a combination of four nucleotides, linked with one another through the pentose molecule, *d*-ribose, which is common to each, and owing its acid character to phosphoric acid, also common to the component

nucleotides. The latter differ from one another in respect of their nitrogenous factors, which are guanine (2-amino-6-oxypurine), adenine (6-aminopurine), uracil (2 : 6-dioxypyrimidine), and cytosine (2-oxy-6-aminopyrimidine), giving their names to the four nucleotides linked with each other in the following manner:

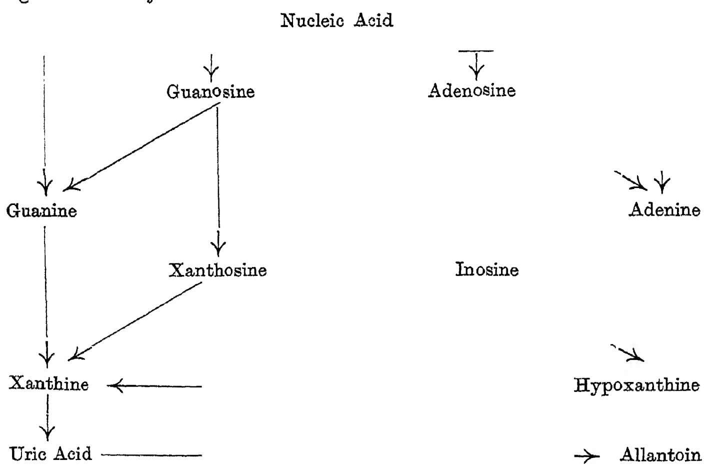
$$\begin{array}{lll}
(HO)_2PO.O.C_5H_7O_2.C_5H_4ON_5 & & \text{(guanine)} \\
\qquad\qquad\quad | & & \\
\qquad\qquad\quad O & & \\
\qquad\qquad\quad | & & \\
(HO)_2PO.O.C_5H_6O.C_4H_4ON_3 & & \text{(cytosine)} \\
\qquad\qquad\quad | & & \\
\qquad\qquad\quad O & & \\
\qquad\qquad\quad | & & \\
(HO)_2PO.O.C_5H_6O.C_5H_4N_5 & & \text{(adenine)} \\
\qquad\qquad\quad | & & \\
\qquad\qquad\quad O & & \\
\qquad\qquad\quad | & & \\
(HO)_2PO.O.C_5H_7O_2.C_4H_3O_2N_2 & & \text{(uracil)}
\end{array}$$

We owe this picture of plant nucleic acid to the combined researches of many chemists, conspicuous amongst whom is A. Kossel; he derived purine bases from nucleins in the early eighteen-eighties, and subsequently identified the products of completely hydrolysing the nucleic acids from yeast and from the thymus gland. Characterisation of intermediate products in such hydrolyses—namely, the nucleotides of guanine, cytosine, adenine, and uracil—with the corresponding nucleosides, guanosine, cytidine, adenosine, and uridine is due chiefly to W. Jones and to P. A. Levene, with whom was later associated W. A. Jacobs; but the most picturesque of all contributions to the subject was made by the earliest of the systematic investigators, Friedrich Miescher, who followed his isolation of nuclein (nucleic acid) from pus cells (1868) by the remarkable discovery that the spermatozoa heads of Rhine salmon consist almost entirely of protamine nucleate (1874), and that this must have arisen, not directly from food, but from muscle protein.

Whilst the yeast cell and the wheat embryo have the power to synthesise nucleic acid of the structure represented above, the thymus gland elaborates another nucleic acid in which a hexose is substituted for *d*-ribose, and uracil is replaced by thymine, its methyl derivative (5-methyl-2 : 6-dioxypyrimidine); the order and mode of nucleotide linkage are also different. These nucleic acids, although deriving their carbohydrate and phosphoric acid from the nourishment on which the organism thrives, do not owe the purine factors to the same source; in other words, the tissues must have power to synthesise a purine ring. The mechanism by which they exercise this power is one of the many problems which await elucidation, but arginine (α-amino-δ-guanidinevaleric acid) has been indicated as one possible origin, whilst histidine (α-amino-β-imidazolylpropionic acid) may be a source of the pyrimidine nucleus.

The transformations undergone by nucleic acid in contact with tissue-extracts have provided the subjects of numerous investigations extending over thirty years. In fact, the experimental material is of

such voluminous complexity as to be unintelligible without the guidance of an expert, and in this capacity W. Jones has rendered valuable service by his recent lucid arrangement of the subject (1921). From this it is comparatively easy to follow the conversion of nucleic acid into uric acid through the agency of enzymes, and a review of these processes can only serve to increase our admiration for the precision and facility with which the chemical operations of the living body are conducted. Regarding for the sake of simplicity only the purine nucleotides, these are probably the first products of hydrolysing nucleic acid, and from them there may be liberated either phosphoric acid by a phospho-nuclease, or the purine-base by a purine-nuclease, giving rise to guanine and adenine, with their nucleosides, guanosine and adenosine. Thereafter the procedure is less obscure. The four products exchange their amino-group for hydroxyl under the influence of their respective deaminase—namely, guanase, adenase, guanosine-deaminase or adenosine-deaminase. The two original nucleosides, with their corresponding derivatives, xanthosine and inosine, are then hydrolysed by their appropriate hydrolase, and the resulting oxypurines, xanthine and hypoxanthine, are further oxidised by xanthine-oxidase to uric acid. This is the concluding phase of purine metabolism in man and apes, but other animals are able to transform uric acid into allantoin by means of uricase. The changes may be represented diagrammatically as follows:—



Considerable progress has been made also in localising the various enzymes among the organs of the body, particularly those of animals. Into the results of these inquiries it is not the purpose of this address to enter further than to indicate that they reveal a marvellous distribution, throughout the organism, of materials able to exert at the proper moment those chemical activities appropriate to the changes which they are required to effect. The contemplation of such a system

continuously, and in health unerringly, completing a series of chemical changes so numerous and so diverse, must produce in every thoughtful mind a sensation of humble amazement. The aspect of this miraculous organisation which requires most to be emphasised, however, is that an appreciation of its complex beauty can be gained only by those to whom at least the elements of a training in chemistry have been vouchsafed. Such training has potential value from an ethical standpoint, for chemistry is a drastic leveller; in the nucleic acids man discovers a kinship with yeast-cells, and in their common failure to transform uric acid into allantoin he finds a fresh bond of sympathy with apes. The overwhelming majority of people arrive at the grave, however, without having had the slightest conception of the delicate chemical machinery and the subtle physical changes which, throughout each moment of life, they have methodically and unwittingly operated.

Chlorophyll and Hæmoglobin.

To those who delight in tracing unity among the bewildering intricacies of natural processes, and by patient comparison of superficially dissimilar materials triumphantly to reveal continuity in the discontinuous, there is encouragement to be found in the relationship between chlorophyll and hæmoglobin. Even the most detached and cynical observer of human failings must glow with a sense of worship when he perceives this relationship, and thus brings himself to acknowledge the commonest of green plants among his kindred. Because, just as every moment of his existence depends upon the successful performance of its chemical duties by the hæmoglobin of his blood corpuscles, so the life and growth of green plants hinge on the transformations of chlorophyll.

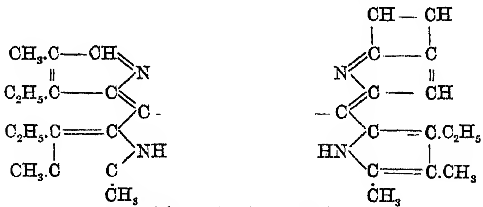
The persevering elucidation of chlorophyll structure ranks high in the achievements of modern organic chemistry, and in its later stages is due principally to Willstätter and his collaborators, whose investigations culminated in 1913. Eliminating the yellow and colourless companions of the substance by a regulated system of partition among solvents, they raised the chlorophyll content to 70 per cent. from the 8 to 16 per cent. found in the original extract, completing the separation by utilising the insolubility of chlorophyll in petroleum ether. By such means, 1 kilogram of dried stinging-nettles gave 6.5 grams of the purified material, representing about 80 per cent. of the total amount which the leaf contains, and application of the process to fresh leaves has established the identity of the product from both sources. Thus the isolation of chlorophyll from plants is now no more difficult than that of alkaloids or of sugars, and may actually be demonstrated as a lecture-experiment.

As a consequence of these operations the dual nature of leaf-green was brought to light in 1912. The focus of main phytochemical action is thus revealed as a system composed of chlorophyll-*a*, bluish-green in solution, and of the yellowish-green chlorophyll-*b*, representing different stages of oxidation as indicated by the formulæ,

$$(a)\ C_{32}H_{30}ON_4Mg(CO_2.CH_3)\ (CO_2.C_{20}H_{39}) = C_{55}H_{72}O_5N_4Mg$$
$$(b)\ C_{32}H_{28}O_2N_4Mg(CO_2.CH_3)\ (CO_2.C_{20}H_{39}) = C_{55}H_{70}O_6N_4Mg$$

In the solid form these products are micro-crystalline powders, bluish-black and greenish-black respectively. They are accompanied by two non-nitrogenous yellow pigments, the unsaturated hydrocarbon, carrotene, $C_{40}H_{56}$, and its oxide, xanthophyll, $C_{40}H_{56}O$, both of which readily absorb oxygen and are allied to a third carrotinoid substance, fucoxanthin, $C_{40}H_{56}O_6$, associated with them in brown algæ and isolated in 1914. Based upon the experience indicated above, Willstätter and his colleagues have examined upwards of 200 plants drawn from numerous classes of cryptogams and phanerogams. The leaf-green of these is identical, and the proportion of *a* to *b* almost invariably approaches 3 : 1 excepting in the brown algæ, in which *b* is scarcely recognisable.

This is not an occasion to follow, otherwise than in the barest outline, the course of laboratory disintegration to which the chlorophyll molecules have been subjected by the controlled attack of alkalis and acids. The former agents reveal chlorophyll in the twofold character of a lactam and a dicarboxylic ester of methyl alcohol and phytol, an unsaturated primary alcohol, $C_{20}H_{39}.OH$, of which the constitution remains obscure in spite of detailed investigation of its derivatives; but the residual complex, representing two-thirds of the original molecule, has been carefully dissected. The various forms of this residual complex, when produced by the action of alkalis on chlorophyll, have been called 'phyllins'; they are carboxylic acids of nitrogenous ring-systems, which retain magnesium in direct combination with nitrogen. The porphyrins are the corresponding products arising by the action of acids; they are carboxylic acids of the same nitrogenous ring-systems from which the magnesium has been removed. The phyllins and the porphyrins have alike been degraded to the crystalline base, ætioporphyrin, $C_{31}H_{36}N_4$, into the composition of which four variously substituted pyrrole rings enter, probably as follows:—



Ætioporphyrin, $C_{31}H_{36}N_4$.

It is this assemblage of substituted pyrroles which, according to present knowledge, is the basic principle also of the blood-pigment, in which iron plays the part of magnesium in chlorophyll. Fundamental as is the difference between hæmoglobin and chlorophyll, relationship can be claimed through this connecting-link, because the same compound, ætioporphyrin, has been produced from hæmoporphyrin, $C_{33}H_{36}O_4N_4$, which is thus its dicarboxylic acid. Hæmoporphyrin arises from hæmatoporphyrin, $C_{33}H_{38}O_6N_4$, produced by the action of hydrobromic acid on hæmin, $C_{33}H_{32}O_4N_4FeCl$, which in turn is derived by exchanging chlorine for hydroxyl in hæmatin,

$C_{33}H_{33}O_5N_4Fe$, the non-albuminoid partner of globin in hæmoglobin. Thus, omitting many intermediate stages, the relationship between chlorophyll and hæmoglobin may be sketched by the following diagram:—

Chlorophyll	Hæmoglobin	→ Hæmatin
		↓
Chlorophyllin	Ætioporphyrin	Hæmin
		↓
Pyrrophyllin		Hæmatoporphyrin
		↓
Ætiophyllin		Hæmoporphyrin

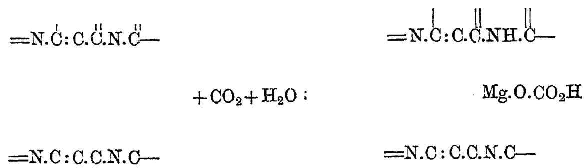
It must be remembered, however, that although recent years have witnessed great progress in elucidating the nature of chlorophyll and hæmoglobin, the mechanism by which they act remains unrevealed. The famous assimilation hypothesis of von Baeyer, according to which it is formaldehyde which represents the connecting-link in the phytochemical synthesis of carbohydrate from carbon dioxide, was enunciated in 1870, and arose from Butlerow's preparation of methylenitan. In spite of numerous criticisms, some of which are quite recent, it remains unshaken. The line of such criticism has taken two directions. On the one hand, H. A. Spoehr (1913), from experiments suggested by the fact that the morning acidity of plant juices diminishes or disappears on exposure to light, has shown that this change is photochemical only, and may be independent of enzymes, the volatile products including formaldehyde. Emil Baur (1908, 1910 and 1913) has urged the claims of oxalic acid to be regarded as the first product of assimilation, and shows how this may lead to the other plant-acids, glycollic, malic and citric, the first-named being a possible stepping-stone to the carbohydrates by resolution into formaldehyde (and formic acid), incidentally assuming towards malic and citric acids the relationship which glucose bears to starch. On the other hand, K. A. Hofmann and Schumpelt (1916), preceded by Bredig (1914), have attacked the hypothesis on the ground of kinetics, and imagine an electrolytic resolution of water under the influence of light, which liberates oxygen and effects the reduction of carbon dioxide by hydrogen to formaldehyde through formic acid.

All these arguments have been weighed by Willstätter and Stoll (1917), who dismiss them on comparing the volume of carbon dioxide absorbed by leaves with the corresponding volume of oxygen liberated. They point out that this assimilatory quotient, CO_2/O_2, which should be unity in the case of formaldehyde, becomes 1.33, 2 and 4 in the case of glycollic, formic and oxalic acids respectively. Proceeding to determine this quotient experimentally they found it to be unity, whether the temperature is 10° or 35°, whether the atmosphere is rich in carbon dioxide or free from oxygen, and alike with ordinary foliage or cactus. Furthermore, they found (1917) that whilst organic liquids holding chlorophyll in solution do not absorb more carbon dioxide than the

liquids themselves, this gas is absorbed much more freely by chlorophyll hydrosols than by other colloidal solutions, a maximum assimilation of two molecular proportions to one magnesium atom being reached, when phæophytin is precipitated:—

$$C_{55}H_{72}O_5N_4Mg + 2CO_2 + 2H_2O = C_{55}H_{74}O_5N_4 + Mg(HCO_3)_2.$$

Prior to this change, which is the first stage appearing in a controlled disruption of chlorophyll-*a* by mineral acids, there is produced an intermediate compound resembling a hydrogen carbonate in which the metal retains a partial grip on the nitrogen:—



It is suggested that leaf-green unites with carbon dioxide by similar mechanism, and that the action of light on the above compound transforms the carbonic acid into an isomeride having the nature of a peroxide such as per-formic acid, H.CO.O.OH, or formaldehyde peroxide, HO.CH<

Anthocyans, the Pigments of Blossoms and Fruits.

Since the days of Eden, gardens have maintained and extended their silent appeal to the more gentle emotions of mankind. The subject possesses a literature, technical, philosophical, and romantic, at least as voluminous as that surrounding any other industrial art, and the ambition to cultivate a patch of soil has attracted untold millions of human beings. Amongst manual workers none maintain a standard of orderly procedure and patient industry higher than that of the gardener. Kew and La Mortola defy the power of word-painters to condense their soothing beauty into adequate language, whilst that wonderful triangle of cultivation which has its apex at Grasse almost might be described as industry with a halo.

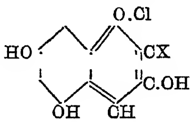
To the countless host of flower-lovers, however, it is probable that Grasse is the only connecting-link between chemistry and their cherished blossoms, they being dimly aware that the ingredients of some natural perfumes have been imitated in the laboratory. The circumstance that identical products of change are generated by the plant, however, and form but one section of the numberless chemical elaborations which proceed before their eyes escapes them because it has been ordained that chemistry is to occupy a backwater in the flood of knowledge. Let us hope that before another century has passed this additional charm to the solace of a garden may be made more generally accessible.

Even to chemists it is only during the last decade that the mechanism of blossom-chemistry has been revealed. The subject has indeed excited their attention since an early period in the history of the organic branch, and the existing class-name for blossom-pigments was first used by Marquart in 1835 to distinguish blue colouring-matters occurring in flowers. It is also interesting to us to notice that in the following year Dr. Hope, who presided over the birth of the Chemistry Section at the Edinburgh meeting in 1834, described experiments conducted with blossoms representing many different orders, and devised a classification of the pigments which they contain. The recognition of glucosides amongst the anthocyans appears to have been first made as recently as 1894, by Heise; about that period, also, it gradually became clear that the various colours assumed by flowers are not variations of a single substance common to all, but arise from a considerable number of non-nitrogenous pigments. Prior to 1913 the most fruitful attempt to isolate a colouring-matter from blossoms in quantity sufficient for detailed examination had been made by Grafe (1911), but the conclusions to which it led were inaccurate. In the year mentioned, however, Willstätter began to publish with numerous collaborators a series of investigations, extending over the next three years, which have brought the subject within the realm of systematic chemistry. For the purpose of distinguishing glucosidic and non-glucosidic anthocyans the names anthocyanin and anthocyanidin respectively were applied. The experimental separation of anthocyanins from anthocyanidins was effected by partition between amyl alcohol and dilute mineral acid, the latter retaining the diglucosidic anthocyanins in the form of oxonium salts and leaving the anthocyanidins quantitatively in the amyl alcohol, from which they are not removed by further agitation with dilute acid; the monoglucosidic anthocyanins were found in both media, but left the amyl alcohol when offered fresh portions of dilute acid.

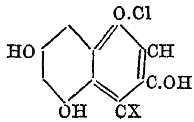
The earliest of these papers, published in conjunction with A. E. Everest, dealt with cornflower pigments, and indicated that the distinct shades of colour presented by different parts of the flower are caused by various derivatives of one substance; thus the blue form is the potassium derivative of a violet compound which is convertible into the red form by oxonium salt-formation with a mineral or plant acid. Moreover, as found in blossoms, the chromogen was observed to be combined with two molecular proportions of glucose and was isolated as crystalline cyanin chloride; hydrolysis removed the sugar and gave cyanidin chloride, also crystalline. Applying these methods more generally, Willstätter and his other collaborators have examined the chromogens which decorate the petals of rose, larkspur, hollyhock, geranium, salvia, chrysanthemum, gladiolus, ribes, tulip, zinnia, pansy, petunia, poppy, and aster, whilst the fruitskins of whortleberry, bilberry, cranberry and cherry, plum, grape, and sloe have also been made to yield the pigment to which their characteristic appearance is due.

The type of structural formula by which the anthocyanidins are now represented was proposed in 1914, simultaneously and

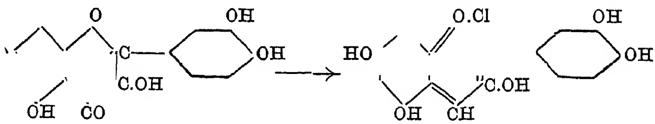
independently by Willsätter and by Everest; incidentally their separate memoirs afford an unusual example of synchronous publication, each having been communicated to the respective academies on the same day, March 26th. Willstätter identified phloroglucinol (1 : 3 : 5-trihydroxybenzene) as a common product of hydrolysing anthocyanidins with alkali, obtaining also *p*-hydroxybenzoic acid from pelargonidin, protocatechuic (3 : 4-dihydroxybenzoic) acid from cyanidin, and gallic (3 : 4 : 5-trihydroxybenzoic) acid from delphinidin. Accordingly he suggested for the anthocyanidin chlorides two alternative formulæ,



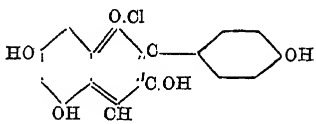
and



in which X represents the substituted benzene ring which appears in the form of a phenolcarboxylic acid on hydrolysis. Later in the same year he confirmed (with Mallison) the former of these representations by reducing quercitin to cyanidin chloride,



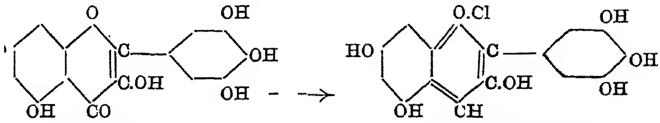
and (with Zechmeister) by effecting a complete synthesis of pelargonidin chloride,



from 2 : 4 : 6-trihydroxybenzaldehyde.

Everest reached the same conclusion by recognising the significance of the fact that a flavone, e.g., luteolin, and a flavonol, e.g., morin, yield red pigments on reduction; he therefore reduced quercitrin (the rhamnoside of quercitin) to cyanidin, and rutin (the rhamnoglucoside of quercitin, and identical with osyritin, myrticolorin, and violaquercitrin) to cyanin. Moreover, he showed that the petals of many yellow flowers, e.g., daffodil, wallflower, tulip, crocus, jasmin, primrose, and viola, or the white blossoms of narcissus, primula, and tulip, all yield red pigments on careful reduction, and in subsequent papers (e.g., with A. J. Hall, 1921) has indicated reduction of yellow sap-pigments belonging to the flavonol group as representing the probable course of anthocyan-formation in plants. In this connection it is noteworthy that an association between the pigments of sap and of blossoms was adumbrated in 1855 by Martens, who suggested that a faintly yellow substance in plant sap, when oxidised in presence of alkalis and light, produces the yellow pigments, and that these, by further oxidation,

change into the red colouring-matters. Everest has shown that reduction is the process actually involved and that flavonols are the precursors of anthocyans, not *vice versa* as suggested by other investigators; moreover, he found (1918) that 'Black Knight' petals contain a glucoside of delphinidin, whilst the corresponding flavonol, myricetin, is present in the sap, also as a glucoside:



Myricetin. **Delphinidin Chloride.**

Hence it will be seen that pelargonidin, cyanidin, and delphinidin, corresponding to the three above-mentioned phenolcarboxylic acids, are the fundamental materials in the group of anthocyan pigments, and that they are derived from the three flavonols, kaempferol, quercitin, and myricetin respectively. The variations upon these types which present themselves in blossoms are twofold, due to (1) the number and position of entrant methyl groups, and (2) the number and character of the aldose molecules which go to form their glucosides. Belonging to the first group are peonidin (monomethylated cyanidin), ampelopsidin, myrtillidin, and petunidin (monomethylated delphinidin) with malvidin and oenidin (dimethylated delphinidin). The second group arise from combination with glucose, galactose or rhamnose, the greatest proportion of pigments occurring as mono- or diglucosides. Thus callistephin and salvinin are the mono- and diglucoside of pelargonidin; asterin and chrysanthemin are monoglucosides and idaein a galactoside of cyanidin, derived from which are the diglucosides cyanin and mekocyanin, and the rhamnoglucoside, keracyanin; violanin is a rhamnoglucoside of delphinidin, whilst delphinin, when hydrolysed with hydrochloric acid, yields delphinidin chloride, glucose and *p*-hydroxybenzoic acid in the molecular proportion, 1 : 2 : 2.

Thus may the chemist find fresh delight in the hedgerow and the garden by reflecting on the processes which lead to molecular structures lying well within his mental horizon, and adorning those familiar models with all the chromatic splendours of snapdragon, pansy, rose, and larkspur. The gustatory and æsthetic thrill engendered in consuming summer pudding and custard is heightened by the soothing blend of egg-yolk lutein and the crimson contributed to the colour scheme by the raspberry and currant anthocyanins.

Micro-Biochemistry.

Amongst the many sources of pleasure to be found in contemplating the wonders of the universe, and denied to those untrained in scientific principles, is an appreciation of infra-minute quantities of matter. It may be urged by some that within the limits of vision imposed by telescope and microscope, ample material exists to satisfy the curiosity of all reasonable people, but the appetite of scientific inquiry is insatiable, and chemistry alone, organic, inorganic, and physical, offers an

instrument by which the investigation of basal changes may be carried to regions beyond those encompassed by the astronomer and the microscopist.

It is not within the purpose of this address to survey that revolution which is now taking place in the conception of atomic structure; contributions to this question will be made in our later proceedings and will be followed with deep interest by all members of the Section. Fortunately for our mental balance the discoveries of the current century, whilst profoundly modifying the atomic imagery inherited from our predecessors, have not yet seriously disturbed the principles underlying systematic organic chemistry; but they emphasise in a forcible manner the intimate connection between different branches of science, because it is from the mathematical physicist that these new ideas have sprung. Their immediate value is to reaffirm the outstanding importance of borderline research and to stimulate interest in submicroscopic matter.

This interest presents itself to the chemist very early in life and dominates his operations with such insistence as to become axiomatic. So much so that he regards the universe as a vast theatre in which atomic and molecular units assemble and interplay, the resulting patterns into which they fall depending on the physical conditions imposed by nature. This enables him to regard micro-organisms as co-practitioners of his craft, and the chemical achievements of these humble agents have continued to excite his admiration since they were revealed by Pasteur. The sixty years which have now elapsed are rich in contributions to that knowledge which comprises the science of microbiochemistry, and in this province, as in so many others, we have to deplore the fact that the principal advances have been made in countries other than our own. On this ground, fortified by the intimate relation of the science to a number of important industries, A. Chaston Chapman, in a series of illuminating and attractive Cantor Lectures in December, 1920, iterated his plea of the previous year for the foundation of a National Institute of Industrial Micro-biology, whilst H. E. Armstrong, in Birmingham a few weeks later, addressed an appeal to the brewing industry, which, although taking the form of a memorial lecture, is endowed with many lively features depicting in characteristic form the manner in which the problems of brewing chemistry should, in his opinion, be attacked.

Lamenting as we now do so bitterly the accompaniments and consequences of war, it is but natural to snatch at the slender compensations which it offers, and not the least among these must be recognised the stimulus which it gives to scientific inquiry. Pasteur's *Études sur la Bière* were inspired by the misfortunes which overtook his country in 1870-71, and the now well-known process of Connstein and Lüdecke for augmenting the production of glycerol from glucose was engendered by parallel circumstances. That acquaintance with the yeast-cell which was an outcome of the former event had, by the time of the latter discovery, ripened into a firm friendship, and those who slander the chemical activities of this genial fungus are defaming a potential benefactor. Equally culpable are those who ignore them. If children

were encouraged to cherish the same intelligent sympathy with yeast-cells which they so willingly display towards domestic animals and silkworms, perhaps there would be fewer crazy dervishes to deny us the moderate use of honest malt-liquors and unsophisticated wines, fewer pitiable maniacs to complicate our social problems by habitual excess.

Exactly how the cell accomplishes its great adventure remains a puzzle, but many parts of the machinery have already been recognised. Proceeding from the discovery of zymase (1897), with passing reference to the support thus given by Buchner to Liebig's view of fermentation, Chapman emphasises the importance of contributions to the subject by Harden and W. J. Young, first in revealing the dual nature of zymase and the distinctive properties of its co-enzyme (1904), next in recognising the acceleration and total increase in fermentation produced by phosphates, consequent on the formation of a hexose-diphosphate (1908):

$$2C_6H_{12}O_6+2Na_2HPO_4=2CO_2+2C_2H_6O+C_6H_{10}O_4(PO_4Na_2)_2+2H_2O.$$

In this connection it will be remembered that a pentose-phosphate is common to the four nucleotides from which yeast nucleic acid is elaborated. The stimulating effect developed by phosphates would not be operative if the cell were not provided with an instrument for hydrolysing the hexose-diphosphate as produced, and this is believed by Harden to be supplied in the form of an enzyme, hexosephosphatase, the operation of which completes a cycle. As to the stages of disruption which precede the appearance of alcohol and carbon dioxide, that marked by pyruvic acid is the one which is now most favoured. The transformation of pyruvic acid into acetaldehyde and carbon dioxide under the influence of a carboxylase, followed by the hydrogenation of aldehyde to alcohol, is a more acceptable course than any alternative based upon lactic acid. Moreover, Fernbach and Schoen (1920) have confirmed their previous demonstration (1914) of pyruvic acid formation by yeast during alcoholic fermentation.

The strict definition of chemical tasks allotted to yeasts, moulds, and bacteria suggests an elaborate system of microbial trades-unionism. E. C. Grey (1918) found that *Bacillus coli communis* will, in presence of calcium carbonate, completely ferment forty times its own weight of glucose in forty-eight hours, and later (1920) exhibited the threefold character of the changes involved which produce (1) lactic acid, (2) alcohol with acetic and succinic acids, (3) formic acid, carbon dioxide, and hydrogen. Still more recent extension of this inquiry by Grey and E. G. Young (1921) has shown that the course of such changes will depend on the previous experience of the microbe. When its immediate past history is anærobic, fermentation under anærobic conditions yields very little or no lactic acid and greatly diminishes the production of succinic acid, whilst acetic acid appears in its place; admission of oxygen during fermentation increases the formation of lactic, acetic, and succinic acids, diminishes the formation of hydrogen, carbon dioxide, and formic acid, but leaves the quantity of alcohol unchanged. The well-known oxidising effect of *Aspergillus niger* has been shown by J. N. Currie (1917) to proceed in three stages marked by

citric acid, oxalic acid, and carbon dioxide, whilst Wehmer (1918) has described the conditions under which citric acid and, principally, fumaric acid are produced by *Aspergillus fumaricus*, a mould also requiring oxygen for its purpose. The lactic bacteria are a numerous family and resemble those producing acetic acid in their venerable record of service to mankind, whilst among the most interesting of the parvenus are those responsible for the conversion of starch into butyl alcohol and acetone. Although preceded by Schardinger (1905), who discovered the ability of *B. macerans* to produce acetone with acetic and formic acids, but does not appear to have pursued the matter further, the process associated with the name of A. Fernbach, and the various modifications which have been introduced during the past ten years are those best known in this country, primarily because of the anticipated connection with synthetic rubber, and latterly on account of the acetone famine arising from the War. The King's Lynn factory was resuscitated and arrangements had just been completed for adapting spirit distilleries to application of the process when, owing to the shortage of raw material in 1916, operations were transferred to Canada and ultimately attained great success in the factory of British Acetones, Toronto.

Much illuminating material is to be found in the literature of 1919-20 dealing with this question in its technological and bacteriological aspects. Ingenuity has been displayed in attempting to explain the chemical mechanism of the process, the net result of which is to produce roughly twice as much butyl alcohol as acetone. The fermentation itself is preceded by saccharification of the starch, and in this respect the bacteria resemble those moulds which have lately been brought into the technical operation of starch-conversion, especially in France. The amyloclastic property of certain moulds has been known from very early times, but its application to spirit manufacture is of recent growth and underlies the amylo-process which substitutes *Mucor Boulard* for malt in effecting saccharification. Further improvement on this procedure is claimed for *B. mesentericus*, which acts with great rapidity on grain which has been soaked in dilute alkali; it has the advantage of inferior proteolytic effect, thus diminishing the waste of nitrogenous matter in the raw material.

Reviewing all these circumstances we find that, just as the ranks of trades-union labour comprise every kind of handicraftsman, the practitioners of micro-biochemistry are divisible into producers of hydrogen, carbon dioxide, formic acid, acetaldehyde, ethyl alcohol, acetic, oxalic, and fumaric acids, acetone, dihydroxyacetone, glycerol, pyruvic, lactic, succinic and citric acids, butyl alcohol, butyric acid. Exhibiting somewhat greater elasticity in respect of overlapping tasks, they nevertheless go on strike if underfed or dissatisfied with their conditions; on the other hand, with sufficient nourishment and an agreeable temperature, these micro-trades-unionists display the unusual merit of working for twenty-four hours a day. One thing, however, they have consistently refused to do. Following his comparison of natural and synthetic monosaccharides towards different families of yeast (1894), Fischer and others have attempted to beguile unsuspecting

microbes into acceptance of molecules which do not harmonise with their own enzymic asymmetry. Various *apéritifs* have been administered by skilled *chefs de cuisine*, but hitherto the little fellows have remained obdurate.

Photosynthesis.

Beyond a placid acceptance of the more obvious benefits of sunshine, the great majority of educated people have no real conception of the sun's contribution to their existence. What proportion of those who daily use the metropolitan system of tube-railways, for instance, could trace the connection between their progress and the sun? Very moderate instruction comprising the elements of chemistry and energy would enable most of us to apprehend this modern wonder, contemplation of which might help to alleviate the distresses and exasperation of the crush-hours.

For many years past, the problem connected with solar influence which has most intrigued the chemist is to unfold the mechanism enabling green plants to assimilate nitrogen and carbon. Although atmospheric nitrogen has long been recognised as the ultimate supply of that element from which phyto-protoplasm is constructed, modern investigation has indicated as necessary a stage involving association of combined nitrogen with the soil prior to absorption of nitrogen compounds by the roots, with or without bacterial co-operation. Concurrently, the agency by which green plants assimilate carbon is believed to be chlorophyll, operating under solar influence by some such mechanism as has been indicated in a preceding section.

Somewhat revolutionary views on these two points have lately been expressed by Benjamin Moore, and require the strictest examination, not merely owing to the fundamental importance of an accurate solution being reached, but also on account of the stimulating and engaging manner in which he presents the problem. Unusual psychological features have been introduced. Moore's 'Biochemistry,' published three months ago, will be read attentively by many chemists, but the clarity of presentation and the happy sense of conviction which pervade its pages must not be allowed to deter independent inquirers from confirming or modifying his conclusions. The book assumes a novel biochemical aspect by describing the life-history of a research. The first two chapters, written before the experiments were begun, suggest the conditions in which the birth of life may have occurred, whilst their successors describe experiments which were conducted as a test of the speculations and are already receiving critical attention from others (e.g., Baly, Heilbron and Barker, Transactions of the Chemical Society, 1921, p. 1025).

It is with these experiments that we are, at the moment, most concerned. The earliest were directed towards the synthesis of simple organic materials by a transformation of light energy under the influence of inorganic colloids, and indicated that formaldehyde is produced when carbon dioxide passes into uranium or ferric hydroxide sols exposed to sunlight or the mercury arc lamp. Moore then declares

that, although since the days of de Saussure (1804) chlorophyll has been regarded as the fundamental agent in the photosynthesis of living matter, there is no experimental evidence that the primary agent may not be contained in the colourless part of the chloroplast, chlorophyll thus being the result of a later synthetic stage. 'The function of the chlorophyll may be a protective one to the chloroplast when exposed to light, it may be a light screen as has been suggested by Pringsheim, or it may be concerned in condensations and polymerisations subsequent to the first act of synthesis with production of formaldehyde' (p. 55). In this connection it is significant that chlorosis of green plants will follow a deficiency of iron even in presence of sunlight (Molisch, 1892), and that development of chlorophyll can be restored by supplying this deficiency, although iron is not a component of the chlorophyll molecule; moreover, green leaves etiolated by darkness and then exposed to light regain their chlorophyll, which is therefore itself a product arising from photosynthesis.

H. Thiele (1907) recorded the swift conversion of nitrate into nitrite by the rays from a mercury quartz lamp, whilst O. Baudisch (1910) observed that daylight effects the same change, and from allied observations was led (1911) to conclude that assimilation of nitrate and nitrite by green plants is a photochemical process. Moore found (1918) that in solutions of nitrate undergoing this reduction green leaves check the accumulation of nitrite, indicating their capacity to absorb the more active compound. Proceeding from the hypothesis that one of the organisms arising earliest in the course of evolution must have possessed, united in a single cell, the dual function of assimilating both carbon and nitrogen, he inquired (1918) whether the simplest unicellular algæ may not also have this power. He satisfied himself that in absence of all sources of nitrogen excepting atmospheric, and in presence of carbon dioxide, the unicellular algæ can fix nitrogen, grow and form proteins by transformation of light energy; the rate of growth is accelerated by the presence of nitrites or oxides of nitrogen, the latter being supplied in gaseous form by the atmosphere. From experiments (1919) with green seaweed (*Enteromorpha compressus*), Moore concluded also that marine algæ assimilate carbon from the bicarbonates of calcium and magnesium present in sea-water, which thereby increases in alkalinity, and further convinced himself that the only source of nitrogen available to such growth is the atmosphere. A description of these experiments, which were carried out in conjunction with E. Whitley and T. A. Webster, has appeared also in the Proceedings of the Royal Society (1920 and 1921).

For the purpose of distinguishing between (1) the obsolete view of a vital force disconnected with such forms of energy as are exhibited by non-living transformers and (2) the existence in living cells of only such energy forms as are encountered in non-living systems, Moore uses the expression 'biotic energy' to represent that form of energy peculiar to living matter. 'The conception, in brief, is that biotic energy is just as closely, and no more, related to the various forms of energy existing apart from life, as these are to one another, and that in presence of the proper and adapted energy transformer, the living

cell, it is capable of being formed from or converted into various of these other forms of energy, the law of conservation of energy being obeyed in the process just as it would be if an exchange were taking place between any two or more of the inorganic forms ' (p. 128). The most characteristic feature of biotic energy, distinguishing it from all other forms, is the power which it confers upon the specialised transformer to proliferate.

Conclusion.

In 'The Salvaging of Civilisation,' H. G. Wells has lately directed the attention of thoughtful people to the imperative need of reconstructing our outlook on life. Convinced that the state-motive which, throughout history, has intensified the self-motive must be replaced by a world-motive if the whole fabric of civilisation is not to crumble in ruins, he endeavours to substitute for a League of Nations the conception of a World State. In the judgment of many quite benevolent critics his essay in abstract thought lacks practical value because it underestimates the combative selfishness of individuals. Try to disguise it as one may, this quality is the one which has enabled man to emerge from savagery, to build up that most wonderful system of colonial organisation, the Roman Empire, and to shake off the barbaric lethargy which engulfed Europe in the centuries following the fall of Rome. The real problem is how to harness this combative selfishness. To eradicate it seems impossible, and it has never been difficult to find glaring examples of its insistence among the apostles of eradication. Why cry for the moon? Is it not wiser to recognise this quality as an inherent human characteristic, and whether we brand it as a vice or applaud it as a virtue endeavour to bend it to the elevation of mankind? For it could so be bent. Nature ignored or misunderstood is the enemy of man; nature studied and controlled is his friend. If the attacking force of this combative selfishness could be directed, not towards the perpetuation of quarrels between different races of mankind, but against nature, a limitless field for patience, industry, ingenuity, imagination, scholarship, aggressiveness, rivalry, and acquisitiveness would present itself; a field in which the disappointment of baffled effort would not need to seek revenge in the destruction of our fellow-creatures: a field in which the profit from successful enterprise would automatically spread through all the communities. Surely it is the nature-motive, as distinct from the state-motive or the world-motive, which alone can salvage civilisation.

Before long, as history counts time, dire necessity will have impelled mankind to some such course. Already the straws are giving their proverbial indication. The demand for wheat by increasing populations, the rapidly diminishing supplies of timber, the wasteful ravages of insect pests, the less obvious, but more insidious depredations of our microscopic enemies, and the blood-curdling fact that a day must dawn when the last ton of coal and the last gallon of oil have been consumed, are all circumstances which, at present recognised by a small number of individuals comprising the scientific community, must inevitably thrust themselves upon mankind collectively. In the

campaign which then will follow, chemistry must occupy a prominent place because it is this branch of science which deals with matter more intimately than any other, revealing its properties, its transformations, its application to existing needs, and its response to new demands. Yet the majority of our people are denied the elements of chemistry in their training, and thus grow to manhood without the slightest real understanding of their bodily processes and composition, of the wizardry by which living things contribute to their nourishment and to their æsthetic enjoyment of life.

It should not be impossible to bring into the general scheme of secondary education a sufficiency of chemical, physical, mechanical, and biological principles to render every boy and girl of sixteen possessing average intelligence at least accessible by an explanation of modern discoveries. One fallacy of the present system is to assume that relative proficiency in the inorganic branch must be attained before approaching organic chemistry. From the standpoint of correlating scholastic knowledge with the common experiences and contacts of daily life this is quite illogical; from baby's milk to grandpapa's Glaxo the most important things are organic, excepting water. Food (meat, carbohydrate, fat), clothes (cotton, silk, linen, wool), and shelter (wood) are organic, and the symbols for carbon, hydrogen, oxygen and nitrogen can be made the basis of skeleton representations of many fundamental things which happen to us in our daily lives without first explaining their position in the periodic table of all the elements. The curse of mankind is not labour, but waste; misdirection of time, of material, of opportunity, of humanity.

Realisation of such an ideal would people the ordered communities with a public alive to the verities, as distinct from irrelevancies of life, and apprehensive of the ultimate danger with which civilisation is threatened. It would inoculate that public with a germ of the nature-motive, producing a condition which would reflect itself ultimately upon those entrusted with government. It would provide the mental and sympathetic background upon which the future truthseeker must work, long before he is implored by a terrified and despairing people to provide them with food and energy. Finally, it would give an unsuspected meaning and an unimagined grace to a hundred commonplace experiences. The quivering glint of massed bluebells in broken sunshine, the joyous radiance of young beech-leaves against the stately cedar, the perfume of hawthorn in the twilight, the florid majesty of rhododendron, the fragrant simplicity of lilac, periodically gladden the most careless heart and the least reverent spirit; but to the chemist they breathe an added message, the assurance that a new season of refreshment has dawned upon the world, and that those delicate syntheses, into the mystery of which it is his happy privilege to penetrate, once again are working their inimitable miracles in the laboratory of the living organism.

SECTION C.—GEOLOGY.

EXPERIMENTAL GEOLOGY.

ADDRESS BY

J. S. FLETT, D.Sc., LL.D., F.R.S.,

PRESIDENT OF THE SECTION.

Among the citizens of Edinburgh in the closing years of the eighteenth century there was a brilliant little group of scientific, literary, and philosophical writers. These were the men who founded the Royal Society of Edinburgh in the year 1783, and many of their important papers appear in the early volumes of its Transactions. Among them were Adam Ferguson, the historian and philosopher; Black, the chemist who discovered carbonic acid and the latent heat of water; Hope, who proved the expansion of water on cooling; Clerk of Eldin, who made valuable advances in the theory of naval tactics, and his brother, Sir George Clerk; Hutton, the founder of modern geology; and Sir James Hall, the experimental geologist. These men were all intimate friends keenly interested in one another's researches. Quite the most notable member of this group was Hutton, who, not mainly for his eminence in geology, but principally for his social gifts, his bonhomie, and his versatility, was regarded as the centre of the circle. Hutton showed an extraordinary combination of qualities. His father was Town Clerk of Edinburgh. After starting as an apprentice to a Writer to the Signet, he took up the study of medicine at the Universities of Edinburgh and Paris, and graduated at Leyden. He then became a farmer on his father's property in Berwickshire, and also carried on chemical manufactures in Leith in partnership with Mr. Davie. He studied methods of agriculture in England and elsewhere, and was an active supporter of the movement for improving Scottish agriculture by introducing the best methods of other countries. A burning enthusiast in geology, especially in the 'theory of the earth,' he travelled extensively in Scotland, England, and on the Continent making geological observations.

His interests were not confined to geology, for he wrote a treatise on metaphysics, which seems to have been more highly esteemed in his day than in ours, and in his last years he produced a work on agriculture which was never published. The manuscript of this work is now in the library of the Edinburgh Geological Society. He also made interesting contributions to meteorology. Hutton's writings are as obscure and involved as his conversation was clear and persuasive, and it is only from the accounts of his friends, and especially Playfair's 'Life of Hutton,' that we can really ascertain what manner of man he was.

It could easily have happened that when Hutton died his unreadable writings might have passed out of notice, to be rediscovered at a subsequent time, when their value could be better appreciated. But Playfair's 'Explanations of the Hutton Theory,' as attractive and

convincing still as when it was originally published, established at once the true position of Hutton as one of the founders of geology. Sir James Hall undertook a different task; he determined to put Hutton's theories to the test of experiment, and in so doing he became the virtual founder of modern experimental geology. It is my purpose in this address to show what were the problems that Hall attacked, by what methods he attempted to solve them, and what were his results. I shall also consider how far the progress of science has carried us since Hall's time regarding this department of geological science.

Hutton was a friend of Hall's father: they were proprietors of adjacent estates in the county of Berwick, and much interested in the improved practice of agriculture, and though the elder Hall (Sir John Hall of Dunglass) has apparently left no scientific writings, he was one of those who were familiar with Hutton's theories and a member of the social group in which Hutton moved. Sir James Hall was the eldest son; born in 1761, he succeeded to the estate on his father's death in 1776. Educated first at Cambridge and then at Edinburgh University, at an early age he became fascinated by Hutton's personality, though repelled by his theories. He tells us how for three years he argued with Hutton daily, rejecting his principles. Hutton prevailed in the long run, and Sir James Hall was convinced. Hall's objection to Hutton's theories is not difficult to understand, though he has not himself explained it. The world was sick of discussions on cosmogony in which rival theorists appealed to well-known facts as proof of the most extravagant speculations. Serious-minded men were losing interest in these proceedings. The Geological Society of London was founded in 1807, and one of its objects is stated to be the avoidance of speculation and the patient accumulation of facts. No doubt Hall also was greatly influenced by the discoveries that Black and Hope had made by pure experimental investigation. His bent of mind was towards chemical, physical, and experimental work, while Hutton was not only a geologist but also a metaphysician.

Foreign travel was then an essential part of the education of a Scottish gentleman, and the connection between France, Holland, and Scotland was closer than it is to-day. Hall travelled widely; in his travels two subjects seem to have especially engrossed him. One was architecture, on which he wrote a treatise which was published in 1813 and is now forgotten. The other was geology. He visited the Alps, Italy, and Sicily. In Switzerland he may have met De Saussure and discussed with him the most recent theories of their time regarding metamorphism and the origin of granites, schists, and gneisses. In Italy and Sicily one of his objects was to observe the phenomena of active volcanoes, and to put to the test of facts the theories of Werner and of the Scottish school regarding the origin of basalt, whinstone, trap, and the older volcanic rocks of the earth's crust. At Vesuvius he made his famous observation of the dykes that rise nearly vertically through the crater wall of Somma, which he held to prove the ascent of molten magma from below through fissures to the surface. This was in opposition to the interpretation of the Wernerians, who regarded them as filled from above by aqueous sediments, and Hall's conclu-

sions, which were strikingly novel at the time, have been abundantly confirmed.

We obtain a pleasant glimpse of Hall's life in Berwickshire in the account of his visit with Hutton and Playfair to Siccar Point in the year 1788. The start was made from Dunglass, where probably the party had spent the night. The great conglomerates of the Upper Old Red Sandstone of that district had much impressed Hutton. He saw in them the evidence of new worlds built out of the ruins of the old, with no sign of a beginning and no prospect of an end—a thesis which was one of the corner-stones of his 'Theory of the Earth.' No doubt Hall knew or suspected that in the cliff-exposures at Siccar Point, where the Old Red rests upon the Silurian, there was evidence which would put this dogma to a critical test.

Hall's first experiments were begun in the year 1790, his object being to ascertain whether crystallisation would take place in a molten lava which was allowed to cool slowly. It was generally believed that the results of fusion of rocks and earths were in all cases vitreous, but glassmakers knew that if glass was very slowly cooled, as sometimes happened when a glass furnace burst, the whole mass assumed a stony appearance. An instance of this had come under Hall's notice in a glassworks in Leith, and its application to geology was clear. Hutton taught that even such highly crystalline rocks as granite had been completely fused at the time of their injection, and their coarse crystallisation was mainly due to slow cooling.

For the purpose of his experiments Hall selected certain whinstones of the neighbourhood of Edinburgh, such as the dolerites of the Dean, Salisbury Crags, Edinburgh Castle, the summit of Arthur's Seat, and Duddingston; but he also used lava from Vesuvius, Etna, and Iceland. He made choice of graphite crucibles, and conducted his experiments in the reverberatory furnace of an ironfoundry belonging to Mr. Barker. As had been shown by Spallanzani, to whose experiments Hall does not refer, lavas are easily fusible under these conditions. Hall had no difficulty in melting the whinstones and obtaining completely glassy products by rapid cooling. He now proceeded to crystallise the glass by melting it again, transferring it from the furnace to a large open fire, where it was kept surrounded by burning coals for many hours, and thereafter very slowly cooled by allowing the fire to die out. He succeeded in obtaining a stony mass in which crystals of felspar and other minerals could be clearly seen. Some of his specimens were considered to be very similar in appearance to the dolerites on which his experiments were made.

The only means of measuring furnace temperatures available at that time were the pyrometers which had recently been invented by Wedgwood. Hall found that a temperature of 28 to 30 Wedgwood yielded satisfactory results. This seems to be about the melting-point of copper, approximately 1000° C.

Whether by design or accident, Hall chose for his experiments precisely the rocks which were most suitable for his purpose. If granite had been selected no definite results would have been obtained. De Saussure had already made fusion experiments on granite. Ninety

years afterwards the problem was completely solved by Fouqué and Lévy, who used a gas furnace and a nitrogen thermometer. They found that it was possible to obtain either porphyritic or ophitic structure by modifying the conditions, and that the minerals had exactly the characters of those of the igneous rocks. Some of Hall's re-crystallised dolerites were examined microscopically by Fouqué and Lévy, and, as might be expected, they proved to be only partly crystallised, showing skeleton crystals of olivine and felspar with grains of iron ore in a glassy base.

Some curious observations made by Hall in his experimental work were also confirmed by Fouqué and Lévy. The crystalline whinstones were more difficult to melt than the glasses which were obtained from them, and the glass crystallised best when kept for a time at a temperature a little above its softening point. It is not possible to assign a definite melting-point to the Scottish whinstones with which Hall worked. Many of them contain zeolites, which fuse readily. Minerals are also present that decompose on heating, such as calcite, dolomite, chlorite, and serpentine. The whole process is very complex, and probably takes place by several stages not sharply distinct. Similarly the glasses cannot be said to have a melting-point. They are really super-cooled liquids. A full explanation of what took place in Hall's crucibles cannot be given at the present day, but there is no room for doubt that his experiments were good and his inferences accurate. His friend Kennedy, who had recently discovered the presence of alkalis in igneous rocks, furnished valuable support to Hall's conclusions by showing that the chemical composition of whinstone and of basalt were substantially identical.

Apparently the results of Hall's work were not received with unmixed approbation. Hutton was distinctly uneasy, and it has been suggested that he feared if experimental work turned out unsuccessful it might bring his theories into discredit. The Wernerians frankly scoffed; they preferred argument to experiment, and the endless discussion went on. Gregory Watt repeated Hall's experiments by fusing Clee Hill dolerite, a hundredweight or two at a time, in a blast-furnace. But there can be no doubt that among those who were not already committed to the principles of Werner the new evidence produced a strong impression, and helped to widen the circle of Hutton's supporters.

Hall's most famous experiments were on the effect of heat combined with pressure on carbonate of lime. The problem was, Can powdered chalk be converted into firm limestone or into marble by heating it in a confined space? In this case Hutton's theories were in apparent conflict with experimental facts; from general observations he held it proved that heat and pressure had consolidated limestones and converted them into marbles. It was well known, of course, that limestone, when heated in an open vessel, was transformed into quicklime, and Black had shown that the explanation was that carbonic acid had been expelled in the form of a gas.

The experiments were begun in 1790, but deferred till 1798 after Hutton's death. Hutton quite openly disapproved of experiments. His

famous apophthegm has often been quoted about those who 'judge of the great operations of the mineral kingdom by kindling a fire and looking in the bottom of a crucible.' In deference to the feelings of his master and his father's friend, Sir James Hall, with admirable self-restraint, decided not to undertake experimental investigations in opposition to Hutton's expressed opinion. With a few months' interruption in 1800 they were continued till 1805. A preliminary account of the results was communicated to the Royal Society of Edinburgh on August 30, 1804, and the final papers submitted on June 3, 1805. Hall states that he made over 500 individual experiments and destroyed vast numbers of gun-barrels in this research.

The method adopted was to use a muffle-furnace burning coal or coke and built of brick. No blast seems to have been employed. The chalk-powder was enclosed in a gun-barrel cut off near the touch-hole and welded into a firm mass of iron. The other end of the barrel could be kept cool by applying wet cloths, and as it was not in the furnace its temperature was always comparatively low. Various methods of plugging the barrel were adopted; at first he used clay, sometimes with powdered flint. Subsequently a fusible metal which melted at a temperature below that of boiling water was almost always preferred. Borax glass with sand was used in some of the experiments, but it was liable to cracking when allowed to cool, and consequently was not always gas-tight. It was essential, of course, that in sealing up the gun-barrel, and in subsequently removing the plug, the temperatures should never be so high as to have any sensible effect on the powdered chalk or limestone. Hall tried vessels with screwed stoppers or lids at first, but never found them satisfactory.

In the gun-barrel there was always a certain amount of air enclosed with the chalk. Very early in the experiments it was shown that if no air-space was provided the fusible metal burst the barrel. No means was found to measure the size of the air-space accurately, but approximately it was equal to that of the powdered chalk used in the experiment. If the air-space was too large, or if there was an escape of gas, part of the chalk was converted into lime.

As each experiment lasted several hours the temperature of the chalk was approximately equal to that of the part of the muffle in which it was placed. Pyrometry was as yet in its infancy. Wedgwood had invented pyrometric cones and Hall had heard of them, but apparently at first he was not in possession of a set. He made his own cones, as nearly similar as possible to those of Wedgwood, and subsequently obtaining a set of Wedgwood's cones he standardised his own by comparison with them. His gun-barrels of Swedish and Russian iron ('Old Sable') were softened, but seldom gave way except when the internal pressures were of a high order. Some of the gun-barrels seem to have been used for many experiments without failure occurring. As Hall made his own pyrometric cones, and we have no details of their composition and the method of preparation, it is not possible to do more than guess at the temperatures to which his powdered lime and chalk were exposed. There is no doubt that by constant practice and careful observation he was able to regulate the temperature within fairly wide limits.

Hall began his experiments as already stated in 1798. They were interrupted for about a year (March 1800 to March 1801), and on March 31, 1801, he had obtained a considerable measure of success. A charge of forty grains of powdered chalk was converted into a firm granular crystalline mass of limestone. The loss on weighing was approximately 10 per cent. Another charge of eighty grains was converted into marble (on March 3, 1801), with a loss of approximately 5 per cent., and the crystalline mass showed distinct rhombohedral cleavage.

Though it cannot be said that his success was easily won he was by no means satisfied, and for another four years he continued his researches. Many different methods were tried in order to ascertain the most satisfactory and reliable; his ambition was to attain complete control of the process so that he could always be certain of the result. Porcelain tubes were tried, which he obtained from Wedgwood. They were very liable, however, to allow escape of the gases through pores. Many different methods of obtaining gas-tight stoppers were experimented on, but he does not seem to have found anything really better than the fusible metal. A slight loss of weight in the chalk used seemed inevitable, and the amount of loss varied irregularly; after long trials he ultimately succeeded in reducing this to less than 1 per cent. Various kinds of carbonate of lime were used, including chalk, limestone, powdered spar, oyster shells, periwinkles, and each of these was crystallised in turn. Many experiments showed that a reaction might take place between the chalk powder and the glass of the tube in which it was contained. The result was a white deposit often crystalline, and a certain amount of uncombined carbonic acid gas which escaped when the tube was opened. No doubt the white mineral was wollastonite. Hall proved that it was a silicate of lime which dissolved in acid and left a cloud of gelatinous silica. Thereafter he used platinum vessels instead of glass to contain the charge of carbonate of lime which he wanted to fuse. The effect of impurities in the material used was also investigated. Critics had urged that his limestone was not pure. Hall aptly replied that this was so much the better; natural limestones were seldom pure, and his point was that limestone might be fused under heat and pressure. He obtained the purest precipitated carbonate of lime, and used also perfectly transparent crystalline spar; the results were, as we might expect, that the pure substances and the fairly coarse crystalline powder were more difficult to fuse than the very finely ground natural chalk. These results show that Hall had very complete control of his experimental processes, and that even small differences in fusibility did not escape his observation.

As natural limestones are always moist, Hall's attention was next directed to the influence of water on the crystallisation of his powders. This added greatly to the difficulty of the experiments, but by wonderful skill he succeeded in using a few grains of water (apparently up to 5 per cent. of the weight of the chalk). The result was to improve the crystallisation, for the reason, as Hall believed, that the pressure was increased. He noticed at the same time that hydrogen was produced, which took fire when the gun-barrel was discharged. Probably there

was also some carbonic oxide. About this time he was using bars of Russian iron into which a long cylindrical cavity had been bored. He then tried other volatile ingredients such as nitrate of ammonia, carbonate of ammonia, and gunpowder. In January 1804 he was able to convert chalk into firm limestone at a temperature about 960° (melting-point of silver) in presence of small quantities of water with a loss of less than one-thousandth part of the chalk used.

Finally he attempted to measure the pressure which was necessary to effect re-crystallisation under the conditions of his experiments. No pressure gauges were available at that date, and after many trials he employed a stopper faced with leather and forced against the mouth of his iron tube by means of weights acting either directly or through a lever. He ultimately succeeded in obtaining gas-tight junctions under pressures ranging from 52 up to 270 atmospheres, and concluded that 52 atmospheres was the least pressure which could be satisfactory. This is equal to the pressure of a column of water 1,700 feet high or to a column of rock 700 feet high. A 'complete marble' was formed at a pressure of 86 atmospheres and carbonate of lime 'absolutely fused' under a pressure of 173 atmospheres.

In reviewing these classic experiments after a lapse of 120 years we feel that there are many points on which we should have liked more detailed information. One essential, for example, is exact chemical analysis of all the materials employed. Even chalk is variable in composition to a by no means negligible extent. Oyster shells and periwinkle shells contain organic matter, which would account for the considerable loss in weight they always exhibited. The use of glass tubes was a defect in the early experiments afterwards remedied by employing platinum vessels. Although in all the experiments the charge was weighed it seems clear that at first at any rate the materials were not carefully dried. In the experiments with water it was seldom possible to provide absolutely against the escape of moisture when the fusible metal was introduced. Most of all we may regret the inadequate means of measuring the temperatures at which the experiments were conducted. The measurements of pressure were made by the simplest possible means, and it was only by great experimental skill and care that even approximate results could be obtained.

Such criticisms, however, do not mar the magnificent success of Hall's experiments. For nearly a hundred years, in spite of the advance of physical and chemical science, no substantial improvement on his results was attained. His work was immediately recognised as trustworthy and conclusive, and became a classic in the literature of experimental geology. Although not exactly the founder of this school of research, for Spallanzani and De Saussure had made fusion experiments on rocks before his time, he placed the subject in a prominent position among the departments of geological investigation, and did great service in supporting Hutton's theories by evidence of a new and unexpected character.

As Hall himself has told us, there were critics who before the complete account of his researches in carbonate of lime was published had challenged the accuracy of some of his conclusions. The ground seems

to have been that the materials he worked with were impure, and that the glass or porcelain vessels in which the powdered chalk was placed were visibly acted on during the experiment. Hall recognised the justice of these conclusions in so far that he made further experiments on the purest precipitated carbonate of lime that he could obtain, and he used platinum vessels instead of glass. These changes admittedly made success more difficult to attain, but he considered that he ultimately was able to fuse the pure chemical in platinum vessels with only a negligible loss of weight by escape of carbonic acid. This seems to have silenced criticism, and with the gradual acceptance of most of Hutton's theories the controversy died down for a time.

Many attempts were made to repeat Hall's experiments during the next eighty years with varying degrees of success. No one was able to secure perfectly gas-tight stoppage of porcelain or iron tubes as Hall did, though they had the record of his experiments to help them, a fact which shows how extremely skilful Hall was in experimental practice. But various authors found that chalk, powdered limestone, and even pure Iceland spar powder or precipitated carbonate of lime could be converted into a firm coherent mass by heating in an open furnace. It was also claimed that lithographic limestone became a crystalline rock resembling marble when heated before a blowpipe under certain conditions. Whether the mass was actually fused was not expressly proved by any of these experiments, and in time it came to be recognised that to make a limestone from powdered chalk it was not necessary that melting should take place. On the other hand, it was contended that when Hall's experiments had resulted in the production of a vesicular or frothy mass which showed evidence that it had dripped or flowed, or that it had been spattered in drops about his apparatus, as he concluded from the appearance presented in certain of his experiments, there was some reason to believe that chemical action had taken place between the silicates of his tubes of glass or porcelain, or the pipeclay stems in which the drops of water were contained, and the carbonate of lime, with the formation of readily fusible compounds. Hall, of course, was perfectly aware that his carbonate of lime combined with the ingredients of glass, porcelain, pipeclay, refractory cones, and silica. He had noticed that in many experiments. What was necessary was a complete quantitative chemical analysis of some of his fused masses, to show that they were carbonate of lime and nothing else. This he never performed. He had his specimens of artificial marble cut and polished, thus testing their hardness, their crystalline structure, and their transparency. He noted also how far the specimens were permanent in dry air, and found that very frequently they disintegrated owing to the presence of a considerable proportion of caustic lime. In many cases also he threw part of the mass into acid and observed complete solution with effervescence of carbonic acid gas. He trusted apparently to determining whether there had been loss of weight, by escape of carbonic acid gas either during the experiment or subsequently on opening the gun-barrel, and argued that if the tube and its contents had the same weight after and before the experiment there could have been no chemical

change. This argument is sound, but confirmatory evidence by quantitative analysis would have rendered the matter certain.

The other question under dispute, viz.: whether actual fusion had taken place or only re-crystallisation in a pasty mass, was of a higher order of difficulty, and neither in Hall's time nor for a century later were means available definitely to settle it.

During the whole of the nineteenth century this controversy lasted and no satisfactory conclusions were reached. Those who tried to repeat Hall's experiments with porcelain tubes, gun-barrels, and iron cylinders met the same difficulties as he did, and were on the whole less successful in overcoming them. In most cases their gun-barrels burst or the method of stopping them failed. It became clear that a coherent mass could be obtained from powdered chalk or pure carbonate of lime at a red heat without great pressure, but no one obtained really convincing evidence of fusion. The problem remained practically as Hall had left it.

The twentieth century, however, has witnessed a tremendous improvement in our methods of tackling such questions as these, and the result has been that a new department of physico-chemical or experimental petrology has been opened up and already possesses a large and most interesting literature. It is really the old experimental geology of Sir James Hall, developed almost beyond recognition. Essentially three factors have produced this result. One is the application of the electric furnace, so powerful and at the same time so compact and easily managed. By its means temperatures from 1000° to 1600° C. are easily obtained, and as many silicates and other minerals have fusion points between those limits their behaviour in the molten state and during crystallisation and cooling becomes accurately observable. The second factor is the invention of the electric pyrometer by which temperatures up to the melting-point of platinum can be observed instantaneously and continuously with an accuracy of one or two degrees centigrade. The third important element which has determined the recent progress of knowledge in this field is the theoretical mathematical researches of such men as Willard Gibbs, Roozeboom, Schreinemakers, and Smits, which are so full and clear that in many respects they are far in advance of the experimental results. To these we may add the continual improvement in microscopic methods of determining minerals, and the advance in knowledge of crystallography, optics, and analytical chemistry.

Among workers in this field it is generally agreed that only the purest chemicals or minerals should be used, as the presence even of traces of impurity may great modify the phenomena, and the interpretation ot the results is so difficult that unnecessary complications must be studiously avoided.

The behaviour of $CaCO_3$ under heat and pressure is really a question of two components CaO and CO_2, one of these being solid and very infusible, the other a gas at ordinary temperatures. We may simplify it by regarding the system for our present purposes as consisting of CaO and $CaCO_3$ with CO_2 as a volatile constituent, arising from the dissociation of $CaCO_3$ at certain temperatures and pressures.

Of the components CaO, lime, is a solid fusible in the electric arc at a temperature about 2570° C. as measured by the optical pyrometer. There are reasons for believing that lime exists in two forms; one of these is nearly isotropic and perhaps amorphous, and is obtained by the dissociation of $CaCO_3$ at low temperatures; the other is cubic with good cleavage and generally occurs in rounded crystals; at high temperatures this is probably the only form met with.

$CaCO_3$ as a mineral and as a chemical compound has been so extensively studied that the literature would fill a considerable library. At least eight forms of it have been described. Four of these, ktypeite, conchite, lublinite, and vaterite are rare and doubtful, and are by no means satisfactorily known. Recently a form known as $\mu CaCO_3$ has been described, but it is not believed to be of importance as a mineral. Two others are the well-known minerals calcite and aragonite. Calcite is the stable form under ordinary conditions. Aragonite is transformed into calcite slowly in presence of moisture and carbonic acid, and rapidly if heated to a temperature from 400° to 500° C., but is practically stable in dry air at ordinary temperatures and pressures. There is some reason for believing that aragonite would be the stable form in temperatures 100° C. or so below the freezing-point. It has a higher specific gravity than calcite. In all experiments in which well-formed crystals of aragonite have been heated they changed to granular crystalline aggregates of calcite before dissociation or melting began. This transition so far as is known is irreversible.

At temperatures between 450° and 970° C. calcite is the result of heating every known form of $CaCO_3$, but above that point it is believed to change to another mineral, $\alpha CaCO_3$, not very different in crystallographic and optical characters. The transition is reversible, and as the temperature falls calcite is again formed. The existence of this transition is indicated by the heating and cooling curves of calcite, which show a discharge of heat delaying the fall of temperature about 970°C. The change is very small. Optical studies have been made with the help of an electric furnace closed with transparent quartz-glass plates, but no measurements were obtained of the optical constants of the mineral, and of its crystallographic form nothing is known except that it is probably trigonal. The change in fact is very similar to that by which quartz passes into α-quartz at a temperature of 575° C., and it has been proposed to use calcite like quartz as a geological thermometer.

Carbonate of lime when heated in a closed vessel melts at a temperature of 1289°. A pressure of not less than 110 atmospheres of carbonic acid gas is necessary to prevent dissociation at the melting temperature. It forms quite a liquid melt which will readily flow through cracks in the platinum vessel that contains it. On cooling the melt crystallisation takes place readily, and the resulting mass when cold is finely granular and completely crystalline.

The dissociation pressure of $CaCO_3$ when heated has been studied by several investigators and the results are somewhat discordant. The most reliable results obtained by actual experiment show that at 587° dissociation has only begun, the pressure of CO_2 being only one millimetre of mercury. At 700° it is 25mm., at 800° about 160mm.,

and at 900° about 720mm., so that it increases rapidly with rise of temperature. The dissociation pressure at the melting-point stated above was determined experimentally. It is quite probable that the high pressure of carbonic gas favours fluidity in the melt, and also accelerates crystallisation.

If the pressure be less than the figures given above, a certain amount of dissociation will take place, and lime, CaO, will be present in the melt. We have then a binary system CaO and $CaCO_3$ and a binary eutectic point is to be expected. Boeke has investigated this system, and finds that the eutectic mixture has a melting temperature approximately 1218° C., and consists of 91 per cent. $CaCO_3$ and 9 per cent. CaO. Small additions of $CaCO_3$ raise the melting-point only slowly, but the presence of additional lime makes the melt far more infusible. Mixtures of CaO and $CaCO_3$ with more than 9 per cent. CaO show on microscopic examination a finely crystalline first generation of lime crystals followed by a second generation of $CaCO_3$ and CaO well crystallised. On the other hand, mixtures containing less CaO than the eutectic proportion show branching skeleton crystals of early $CaCO_3$ which have a development indicating trigonal symmetry, and a ground mass consisting of CaO and $CaCO_3$. The large early skeleton crystals often continue to grow during the consolidation of the eutectic so as to give a coarsely crystalline appearance to the aggregate. Hence melts containing little lime often yield semi-transparent crystalline masses having the appearance of marble though not its minute structure. The refractive index of CaO obtained in this way is about 1.83, and it is optically isotropic, so that there is no difficulty in recognising it in the microscopic slides.

So far as research has yet gone, no evidence has been found that there are intermediate compounds between CaO and $CaCO_3$, and the two substances do not appear to form solid solutions to a perceptible extent.

To indicate how far experimental methods have advanced since the days of Sir James Hall, a brief account of the apparatus used will not be without interest. The carbonate of lime was either true Iceland spar, which is quite as free from admixture as the best prepared carbonate, or specially purified precipitated $CaCO_3$. It was heated in a platinum vessel, as in Hall's experiments. This vessel was placed in a small electric resistance furnace with walls of fireclay and magnesia and a platinum spiral. This furnace could attain a temperature of 1600° in a few minutes, and maintain it perfectly steadily for days if required. The small furnace containing the platinum tube was now placed in a steel vessel less than six inches in diameter with thick walls. The lid of the container was fastened with bolts and nuts and a lead washer used to prevent escape of gas. By this arrangement the small internal furnace was alone heated; the steel enclosing vessel could be kept cold if necessary by a water-cooling arrangement, and it was a fairly simple matter to obtain gas-tight connections, and to obviate any risk of bursting. The space between the steel vessel and the furnace was packed with purified asbestos, to prevent convection currents in the carbonic acid gas from affecting the temperature of the electric furnace.

Temperature was measured by a platinum-platinum-rhodium electric pyrometer of which the sensitive part was immersed in the $CaCO_3$ which was being experimented on. Carbonic acid gas was provided in an ordinary steel cylinder such as is used for trade purposes; these can easily stand higher pressures (at ordinary temperatures) than those it was necessary to employ. The gas in these cylinders is at fifty atmospheres pressure, but by immersing the cylinder in hot water the pressure could be raised sufficiently for the purposes of the experiment. All pressures were measured by an ordinary Bourdon gauge, such as is used for many purposes in the arts. Through the walls of the vessel the insulated wires of the electric furnace and pyrometer and the tube carrying carbonic acid gas were led by gas-tight junctions. The whole apparatus worked perfectly smoothly. It is a type of experimental plant which is already employed in many researches into the behaviour of substances at high temperatures under considerable gas pressures, and seems likely to play a large part in the progress of experimental geology in the near future. By slow stages it has reached its present development, and when we remember how many advantages we enjoy in experimental work to-day as compared with Sir James Hall, who was a real pioneer and had to invent all his apparatus and solve every difficulty for himself, we can appreciate more thoroughly the masterly ingenuity he displayed.

As the outstanding uncertainty about Hall's experiments is the question whether his carbonate of lime was actually melted or not, we may pause to consider what evidence is accepted as sufficient on this point at the present day. In ordinary cases the proof of fusion would be that the mass became liquid, but as the charge is contained in a furnace inside a closed steel vessel it is impossible to examine it till the apparatus cools down and is opened up. In many cases also it is possible to rapidly cool the melt, by dropping it into water or mercury, and if it solidifies as a pure glass the proof of fusion is complete. The carbonate of lime, however, could not be chilled either directly or indirectly, and, furthermore, it seems clear that this substance crystallises so readily that to obtain solidification as a vitreous mass might be quite impossible. Reliance accordingly must be placed on a third experimental method, that of reading the heating and cooling curves as recorded by the pyrometer. Change of state involves either the liberation or absorption of heat, and these may be ascertained without any difficulty. Fortunately the behaviour of carbonate of lime in this respect is quite satisfactory; it melts sharply at a definite temperature and crystallises very readily on cooling, so that the exact fusion point is not difficult to observe. Moreover, the microscopic appearance of the crystalline masses produced is entirely in accordance with the belief that complete fusion had taken place.

We may now consider what light modern research has thrown on the vexed question whether Hall succeeded in melting carbonate of lime, and on the value and accuracy of his experimental work generally. It is clear that Hall in his best experiments was able to prevent escape of gas from his gun-barrels. The fact that there was no significant loss of weight in the materials he used seems to prove this satisfactorily.

His latest experiments with arrangements for measuring the pressure were conducted in a manner far less likely to obtain complete retention of the gas than his early experiments, but they show that he had obtained a useful first approximation to the pressures involved, and that somewhere between 100 and 150 atmospheres was the dissociation pressure of $CaCO_3$ on melting. So crude was his apparatus, to modern ideas, that it is wonderful he obtained any results at all.

The necessity of providing an air space to prevent bursting of the gun-barrels by expansion of the fusible metal makes it certain that lime was always present in his melt, and as the air space was never accurately measured it is not certain to what extent dissociation took place. He was working therefore with mixtures of CaO and $CaCO_3$, and the temperature of the fusion at which he aimed was the eutectic point at 1218°C. In most cases he probably had excess of lime in his melt, but in his best experiments he was either very near the eutectic mixture or on the carbonate of lime branch of the curve. The effect of the water which he introduced may have been to lower the melting-point slightly though there are no very exact experimental results at the present time to indicate the magnitude of this effect; probably it was not very great. If then he was able to reach a temperature of 1218°C. he may be said to have succeeded. Everything depends on the conditions in his furnace, and this was determined by the design of the furnace, the nature of the fuel, and the draught. Apparently he did not use a blast, and we are not informed as to the chimney. His furnace and muffle seem to belong to a pattern which has been long employed for refining silver and gold and for assaying copper. Now copper melts at 1082°C., and it is open to doubt whether a muffle-furnace of this type will give a temperature of 1218°C. It seems just possible that the melting-point of carbonate of lime may have been actually reached under the best working conditions. The question will never be settled; Hall's pyrometers were the least satisfactory part of his apparatus, and all his critics are agreed that it is impossible to interpret the results that they gave. Without an exact knowledge of the materials, structure, and dimensions of his furnace, his fuel and his draught, we cannot reproduce the conditions under which he was working, and his descriptions of his methods are too incomplete to settle the point.

The determination of the actual melting-point and vapour pressure of $CaCO_3$ is a question, however, which interests the physical chemist more than the geologist, and there is little evidence to show that the eutectic mixture of lime and carbonate of lime has a distinct importance as a component of rocks. Though Sir James Hall may not have clearly realised it, he had established a truth of far higher value to geologists. He had shown that at comparatively low temperatures such as the melting-point of silver, which is 960°C., a fine grained aggregate of calcite will readily recrystallise. If the mineral is an incoherent powder it will agglutinate into a firm coherent mass. At slightly higher temperatures it becomes plastic, so as to assume the shape of the vessel which contains it, and loses any angularities or irregularities of its surface. These temperatures are about the same as those exhibited by ordinary lavas, and must be quite common in the vicinity of under-

ground intrusions. The whole of the phenomena of the contact alteration of limestone, including the disappearance of original structures and organic remains, find a simple explanation through his experiments. Granted only a temperature about 1000°C. and sufficient pressure to retain the carbonic acid evolved (less than 1000 feet of average rock) any limestone will recrystallise completely. As a matter of fact, there is little evidence that the complete fusion of limestone is a common phenomenon, and a liquid limestone magma sending intrusive veins into the surrounding rocks has only seldom been postulated. The Huttonians thought that the calcareous amygdales of many of the basaltic lavas were fragments of limestone that had been involved in the igneous rock and completely fused, but this is no longer believed. Furthermore, Sir James Hall proved that under the same conditions limestone would react on silica, forming silicates, and would attack glass, porcelain, pipe-clay, and the material of his pyrometric cones; thus he explained the origin of accessory minerals of many limestones, such as wollastonite, garnet, vesuvianite, diopside, and scapolite. Edinburgh geologists, for example, know well the altered limestone which occurs at the margin of the teschenite-picrite sill at Davidson's Mains railway station. There is no need to believe that it was ever completely melted, and the preservation of many traces of the original bedding makes it very improbable that complete fusion took place.

Recrystallisation and the growth of crystals *in solido* were observed also by many of those who endeavoured to repeat Sir James Hall's experiments during the nineteenth century, and have been fully confirmed by more recent researches. In fact this process is now regularly applied in the investigation of minerals that refuse to crystallise well from igneous melts or undergo transformation into other forms below a certain transition temperature. From the pure chemical components a glass is prepared as homogeneous and free from bubbles as possible, and this glass is then heated for many hours to a temperature below its melting-point, but within the field of stability of the crystalline form which it is desired to investigate. Crystals are thus produced which may be sufficiently large to have their optical characters, cleavage, hardness, and other properties satisfactorily determined. This is, of course, a case of devitrification, a process which Hall was familiar with, as he had studied it in the glass furnace at Leith which first suggested to him the advisability of making furnace experiments on rocks. He recognised it also in certain varieties of porcelain which he had employed in his experiments. But even when devitrification is sensibly complete and a finely crystalline aggregate replaces the original glass the process will go on, and the crystals become larger and larger if subjected for a considerable time to a temperature not far below the fusion point.

It was characteristic of Hall that having set himself an object he pursued it with undeviating persistence. For four years he continued his experiments on the crystallisation of the carbonate of lime by heat modified by compression. In that time he made nearly five hundred experiments, and considering how elaborate they were and how all his apparatus was made by himself or by ordinary mechanics we can see that little time was left for the ordinary pursuits of a country

gentleman. A few subsidiary investigations, however, received attention, one being the action of organic matter when heated under pressure, including the formation of coal and the origin of the bituminous materials found where igneous rocks are intrusive into coal seams or beds of shale rich in organic matter. The other was the action of carbonate of lime on 'silex.'

It is not quite clear what the 'silex' was, as Hall employs the term for the material of which his Wedgwood porcelain tubes were made, while others used it to designate precipitated silica and various siliceous minerals. If it were porcelain, Hall was experimenting with the system CaO-Al_2O_3-SiO_2 on which the beautiful researches of Rankin and Wright executed at the Geophysical Laboratory in Washington were published in 1915. This work may be taken as an example of the highest type of investigations of the class which Hall initiated. About 7000 individual tests were made. The complete ternary diagram contains fourteen separate stability fields, each for a definite chemical compound. Some of these are well-known minerals such as cristobalite, tridymite, sillimanite, anorthite, but many are new compounds, or minerals under forms which do not occur in rocks (such as pseudo-wollastonite). In addition to the three components there are nine binary compounds; three are ternary, but of these only two are stable. Between the stability fields are thirty boundary lines, which show under what conditions two minerals may exist simultaneously in the presence of liquid melt of a definite composition. The fields meet three together, in twenty-one quintuple points, eight of which are ternary eutectics, while thirteen are transition points. The lowest temperature at which liquids appear is 1170°C. ± 5°; no possible mixture of these three substances is completely fused below that temperature.

Many binary systems and quite a number of ternary systems have now been explored, some of them very fully. The seed which Hall planted is growing into a mighty tree. It is bearing fruit most precious to petrologists, mineralogists, and physical chemists. The conditions under which certain minerals can form in igneous melts are being gradually determined. But as yet the results appeal to the mineralogist and physical chemist rather than to the geologist. Quartz, tridymite, calcite, pyrites, corundum, and other common minerals of rocks have now had their stability conditions determined when they occur in dry fusions at atmospheric pressure in presence of a limited number of other substances. The accuracy of the determinations is marvellous, and has been confirmed in many cases by independent investigations along different lines. These researches are of even more value to the technologist than to the geologist. In the system above mentioned, for instance, there are only three or four minerals among the compounds determined in the melts. But the whole system and all its compounds have a bearing on practical problems. The three pure substances, for example, are well-known refractories: silica, alumina, and lime. Silica is the material of the quartz-glass industry; alumina forms alundum, an abrasive and a valuable refractory, while the uses of lime are too many to mention. Silica brick is principally quartz and tridymite with a little lime as a bond; ganister brick is mainly silica

and alumina; fireclay contains more alumina with a small and variable amount of alkalis. Portland cement is a mixture of silica, alumina, and lime. All these manufactures are produced by pure dry fusion under atmospheric pressure, and exactly under the conditions and temperatures of the experiments on which the diagram is founded. The presence of small amounts of impurities in the natural minerals employed introduces complications, but these may be neglected if only approximate results are aimed at. During the War the highly trained technical skill of the workers of the Geophysical Institute and their refined apparatus were entirely at the service of American industries, such as the manufacture of optical and chemical glass, and all the practical problems that rose were promptly and satisfactorily solved.

The investigation of the system $Ca\text{-}Al_2O_3\text{-}SiO_2$ which we have taken as an example of the best type of modern work in this field of research is a great contribution to theoretical petrology. It has bearings on the thermal alteration of many rocks such as quartzites, flints, pure limestones, siliceous and argillaceous limestones, bauxites, fireclays, calcareous quartzites, all of which may be regarded as mixtures of silica, clay, and (carbonate of) lime together with their alteration products. But for the geologist as a rule the matter is not quite so simple, and caution is necessary in drawing inferences. Three of the commonest alteration products in this group of rocks, for example, are biotite, garnet, and andalusite, and these minerals have been produced experimentally only under very exceptional conditions.

There are differences between the conditions of the experiments and those that actually obtain in the making of rocks, and these are essentially of three kinds.

(*a*) Experimental work is successful only when the conditions are exactly defined, and necessity compels us at present to restrict experimental work to simple systems of two or three components. The theory of these has been very fully worked out, and this is essential to the interpretation of the experimental results. Systems of four components have hardly yet been touched. If we take, for example, the four common oxides of rocks, CaO, Al_2O_3, MgO, and SiO_2, the six possible binary systems are pretty well known, and the four possible ternary systems have also been thoroughly studied, but little progress has yet been made with the investigation of quaternary mixtures containing all four components. The mathematics of such a system is of the most complex description. Now the common rocks contain seven or more components, and their behaviour in igneous melts is a problem which is at present beyond solution.

To simplify matters we might investigate such a system piecemeal, that is to say, we might take parts of it and treat them as independent systems. For example, the three minerals anorthite, forsterite, quartz, which consist of these four components, have been investigated, and it was proved that this could not be regarded as a simple three-component system, as under certain conditions phenomena appeared which characterised a quaternary mixture. Along these lines, however, there is no doubt that great progress can be made, and the results already obtained are so valuable that they hold out great promise for the future.

(*b*) The only igneous rocks that consolidate from high temperatures at atmospheric pressures with free escape of contained gases are the volcanic lavas. The plutonic and intrusive rocks consolidate under high pressures and with retention of their gases. Hall began his experiments with dry melts in open furnaces; but he realised that under these conditions it was not possible to crystallise a marble. The historical development of research has followed similar lines, and investigations under pressure are now becoming more prominent. In the special field in which Hall worked we owe very important results to Professors Adams and Nicolson, of Montreal. They experimented on the effects of very high pressure (obtained by a hydraulic press) on chalk, limestone, and marble. Columns of limestone were embedded in alum or fusible metal enclosed in a steel tube, and submitted to enormous pressures. In some of the experiments the apparatus was heated to 300° or 400°C., and that the investigation was on 'the effects of heat modified by compression' as stated in the title of Hall's original paper of 1805. They succeeded in obtaining plastic deformation in the solid rock, with development of schistosity and cataclastic structures but without extensive recrystallisation. These experiments illustrate very perfectly the formation of such rocks as calc-schists, mylonites, flaser-gabbros, and augen-gneisses.

It is generally agreed by physicists that increase of pressure makes little difference on the melting-points of solids, and that a slight rise of temperature may have a much greater effect on the stability of a mineral system than a considerable rise of pressure. But this is to some extent altered where volatile substances are concerned, for then pressure modifies the concentration often to a high degree. In many rocks, and especially the acid plutonic rocks and mineral veins, the importance of volatile mineralisers is abundantly clear. In the crystalline schists, on the other hand, we see the effects of pressure, not only in the structures of the rock masses, but also in the special minerals which characterise this group. The importance, accordingly, of pressure and volatile components cannot be ignored, and experimental petrologists are now directing their attention especially to a study of their influence. The ground has been cleared by a masterly series of mathematical researches, principally by Schreinemakers and Smits, and experimental work along these lines is rapidly advancing.

(*c*) The third agency which nature employs in the making of rocks but is apt to be neglected in the laboratory is *time*. It is not always easy to estimate its importance. A laboratory experiment under exceptional circumstances has been carried on over several months; most of them are finished in a few hours, but nature works with unlimited time. The action of solvents when they occur in very small quantities is favoured in this way and unstable phases tend to disappear. In the deposition of mineral veins this may be a factor of paramount importance, and it also cannot be ignored in all studies of metamorphism and metasomatism.

We learn from Hall's papers that he was continually experimenting, and he delighted to devise means to put geological theories to practical tests.

In 1812, when he was President of the Royal Society of Edinburgh, he read a paper to the Society on 'The Contortions of the Strata.' He described the Silurian rocks of the Berwickshire coast as having been thrown into folds, a great part of which had been removed by denudation. Similar phenomena had been noted in many other places, and many explanations had been offered to account for the tilted, bent, upturned, and distorted rocks. Some geologists held they had been deposited in that position, others that they had been upheaved by earthquakes, or let down by subsidences. Hall's explanation was very simple—the rocks had been affected by lateral pressure. With some pieces of cloth and a door 'which happened to be off its hinges,' and a few stones to act as weights, he was able to reproduce the 'contortions of strata' experimentally with great perfection. The whole proceeding was so simple that one is reminded of Columbus and the egg. Yet if we are to judge by the discussions in contemporary literature, folding of strata had not yet been recognised, and this suggestion was revolutionary. It contains the germ of many theories of mountain building; no one now doubts that lateral pressure is one of the most powerful agencies in the disturbance of the earth's crust and the production of many special types of rocks. In no district is this better exemplified than in the North-West Highlands of Scotland.

As the source of lateral pressure Hall suggested that igneous intrusions making their way upwards might force asunder the adjacent rocks. This would not now be generally accepted, but of course it was an explanation very likely to occur to a Huttonian. It seems to have been about forty years later that Elie de Beaumont and his school brought forward the hypothesis that secular contraction of the earth's crust might produce lateral compression of rock masses, and might be the cause of folding and of mountain building. The rise of modern theories of mountain structure probably dates from the investigations of H. D. Rogers on the Appalachians.

Hall's final contribution to experimental geology appears in a paper which he read to the Royal Society of Edinburgh in April 1825, and was published in the tenth volume of the 'Transactions.' The title is 'On the Consolidation of the Strata of the Earth.' Modern geology by that time had made great progress, and many of the controversies of Hall's early years had been settled. But Hall remained essentially a Huttonian in his belief in the efficacy of plutonic heat. He aimed at showing that submarine intrusions would consolidate loose overlying beds of sand into firm sandstone. For this purpose he took salt water, or concentrated brine, and heated it in crucibles or iron vessels containing a quantity of sand. He found that it was possible to make the bottom of such an iron vessel red hot, while the brine on top was so cold that the hand could be inserted into it. The sand was in some cases converted into firm coherent mass, no doubt by the action of alkalis at a red heat. It is difficult to perceive what such an experiment proves; it may possibly have some bearing on the induration of sandstones by contact alteration in the vicinity of intrusive sills, and one of the special cases to which Hall refers as suggesting this experiment was the hardening of conglomerate by intrusive dykes. What actually

happened was the production of a vitreous cement, consisting of silicates of alumina and the alkalis, sufficient to bind together the sand grains. We are reminded of the fused Torridon sandstone that is found at the margins of Tertiary dykes in the western isles of Scotland, but in these the alkali was furnished by the felspar originally present in the sandstone. This paper probably contains the last expression of the pure Huttonian philosophy. Hall was now the sole survivor of the original group who established the Huttonian theories. Having begun as an innovator and a radical, regarded askance for his revolutionary tendencies, he was now a conservative holding fast to orthodox opinions, even when they were out of date. His passion for experiment lasted to the end, and he seems to have maintained his furnace in working order for over forty years. He died in 1832. Although he was not the greatest of the trio—Hutton, Playfair, and Hall—who founded modern geology, he was worthy to take his place with the others. Hutton's was the original master-mind who, by sheer induction and abstract reasoning, had read the secrets of the earth. Playfair was the man of balanced judgment who grasped the essentials and placed them in convincing clearness before a sceptical public. Hall had his special field of work in which he excelled all his contemporaries, and for us who are watching with profound interest the rapid progress of experimental investigation into geophysical and petrophysical problems, it is not uncongenial to pay a tribute to his memory.

SOME PROBLEMS IN EVOLUTION.

ADDRESS BY

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PRESIDENT OF THE SECTION.

It was nearly 100 years ago that Charles Darwin began his scientific studies in the University of Edinburgh. In this illustrious centre of intellectual activity he met various friends keenly interested in natural history, and attended the meetings of scientific societies, and it was doubtless here that were sown many of the seeds destined to bear such glorious fruit many years later. No more fitting subject, I think, could be found for an address than certain problems relating to his doctrine of evolution. That controversy perpetually rages round it is a healthy sign. For we must take care in science lest doctrine should pass into dogma, unquestioned and accepted merely on authority. So from time to time it is useful to re-examine in the light of new knowledge the very foundations on which our theories are laid.

Perhaps the best way of treating these general subjects is by trying to answer some definite questions. For instance, we may ask: 'Why are some characters inherited and others not?' By characters we mean all those qualities and properties possessed by the organism, and by the enumeration of which we describe it: its weight, size, shape, colour, its structure, composition and activities. Next, what do we mean by 'inherited'? It is most important, if possible, clearly to define this term, since much of the controversy in writings on evolution is due to its use by various authors with a very different significance—sometimes as mere reappearance, at other times as actual transmission or transference from one generation to the next. Now, I propose to use the word inheritance merely to signify the reappearance in the offspring of a character possessed by the ancestor—a fact which may be observed and described, regardless of any theory as to its cause. Our question, then, is: 'Why do some characters reappear in the offspring and others not?'

It is sometimes asserted that old-established characters are inherited, and that newly-begotten ones are not, or are less constant, in their reappearance. This statement will not bear critical examination. For, on the one hand, it has been conclusively shown by experimental breeding that the newest characters may be inherited as constantly as the most ancient, provided they are possessed by both parents.[1] While, on the other hand, few characters in plants can be older than the green colour due to chlorophyll, yet it is sufficient to cut off the light from a germinating seed for the greenness to fail to appear. Again, ever since Devonian

[1] We purposely set aside complications due to hybridisation and Mendelian segregation, which do not directly bear on the questions at issue.

times vertebrates have inherited paired eyes; yet, as Professor Stockard has shown, if a little magnesium chloride is added to the sea-water in which the eggs of the fish Fundulus are developing, they will give rise to embryos with one median cyclopean eye! Nor is the suggestion any happier that the, so to speak, more deep-seated and fundamental characters are more constantly inherited than the trivial or superficial. A glance at organisms around us, or the slightest experimental trial, soon convinces us that the apparently least-important character may reappear as constantly as the most fundamental. But while an organism may live without some trivial character, it can rarely do so when a fundamental character is absent, hence such incomplete individuals are seldom met in Nature.

Yet undoubtedly some characters reappear without fail and others do not. If it is neither age nor importance, what is it that determines their inheritance? The answer is that for a character to reappear in the offspring it is essential that the germinal factors and the environmental conditions which co-operated in its formation in the ancestor should both be present. Inheritance depends on this condition being fulfilled. For all characters are of the nature of responses to environment;[2] they are the products or results of the interaction between the factors of inheritance (germinal factors) and the surrounding conditions or stimuli. This power of response or reaction is no mysterious property of organisms—it is the effect produced, the disturbance brought about by the application of a stimulus. All the special properties and activities of living organisms ultimately depend on their metabolism, of which growth and reproduction are the chief manifestations. The course of metabolism, and, consequently, the development in the individual of a character, is moulded or conditioned by the environmental stimuli under which it takes place. On the other hand, the living substance, protoplasm, which is undergoing metabolism is the material basis of the organism. It has a specific composition and structure peculiar to the particular kind of organism concerned, and this is handed on to the offspring in the germ-cells from which starts the new generation. The inheritance of a character is due, then, not only to the actual transmission or transference of this specific 'germ-plasm' containing the same factors of inheritance (germinal factors) as those from which the parent developed, but also to this factorial complex developing under the same conditions (environmental stimuli), as those under which the parent developed. Any alteration either in the effective environmental stimuli or in the germinal factors will produce a new result, will give rise to a new character, will cause the old character to appear no longer.

Now what is actually transmitted from one generation to the next is the complex of germinal factors. Hence we should carefully distinguish between transmission and inheritance. Much of the endless

[2] In a letter to *Nature* Sir Ray Lankester long ago drew attention to the importance of this consideration when discussing inheritance. He also pointed out that Lamarck's first law, that a new stimulus alters the characters of an organism, contradicts his second law, that the effects of previous stimuli are fixed by inheritance. (*Nature*, vol. li. 1894.)

confusion and interminable controversies about the inheritance of so-called 'acquired characters' is due to the neglect of this important distinction. For it is quite clear that whereas factors may be transmitted, characters as such never are. The characters of the adult, being responses, are not present as such in the fertilised ovum from which it develops, they are produced anew at every generation.[3] No distinction in kind or value can be drawn between characters.

If some are inherited regularly and others are not, the distinction lies not in the nature or mode of production of the characters themselves, but in the constancy of the factors and conditions which give rise to them. Thus, although there is only one kind of character, there are two kinds of variation.

Much of the confusion in evolutionary literature is, I think, due to the use of the word variation in a loose manner. Sometimes it is taken to mean the degree of divergence between two individuals; sometimes the character itself in which they differ, such as a colour or spot on a butterfly's wing, at other times a variety or race differing from the normal form of the species. If clearness of thought and expression is to be attained, the word variation should mean the extent or degree of difference between two individuals or between an individual and the average of the species, the divergence of the new form from the old; not a new character or assemblage of characters, but a difference which can be measured or at least estimated. We shall then find that a variation is of one of two kinds (which may, of course, be combined): the first kind is due to some change in the complex of effective environmental stimuli, the second to some change in the complex of germinal factors.

The second kind, to which the name mutation has been applied, will, under constant conditions, be inherited since the new complex of factors will be transmitted to subsequent generations. The first kind of variation, which has been called a modification, will also be inherited, provided, of course, the change of stimulus persists. In either case, new characters will result. But here, again, we must be careful not to apply the terms mutation and modification to the characters themselves, as is so often done;[4] for we then reintroduce the confusion already exposed in the popular but misleading distinction between 'acquired' and 'non-acquired' characters. The characters due to mutation or modification are, of course, indistinguishable by mere inspection, and can only be separated by experiment. A mutation once established should give rise, under uniform conditions, to a new heritable character, and may be detected by crossing with normal members of the species.

[3] In other words, all characters are 'acquired during the lifetime of the individual,' and 'inherited' in the sense here defined has just the same meaning. Much the same view was advocated by Professor A. Sedgwick in his address to this Section at Dover in 1899, and it has also been developed by Dr. Archdall Reid and others.

[4] The name 'mutation' might be given to the alteration in the factors instead of the variation due to it. The latter might then be termed a mutational variation and would be opposed to a modificational variation. At present the term 'mutation' is applied to three different things: the factorial change, the variation or difference, and the new product response or character.

So far observations and tests have shown that new characters due to modification only reappear so long as the new stimulus persists. The difference lies not in the value or permanence of the new character, but in the causes which give rise to it.[5]

It is little more than a platitude to state that, for the production of an organism or of any of its characters, both germinal factors and environmental stimuli are necessary, and that if evolution is to take place there must be change in one or both. Yet the changes in the factors may be held to be the more important. In an environment which on the whole alters but little, evolution progresses by the cumulation along diverging lines of adaptation of new characters due to mutation. Thus natural selection indirectly preserves those factorial complexes which respond in a favourable manner. In other words, an organism, to survive in the struggle for existence, must present that assemblage of factors of inheritance which, under the existing environmental conditions, will give rise to advantageous characters.

In answer to a further question, let us now try to explain what we mean when we contrast the organism with its environment. In its simplest and most abstract form a living organism may be likened to a vortex. That mixture of highly complex proteins we call protoplasm, the physical basis of life, is perpetually undergoing transformations of matter and energy, so long as life persists. Towards the centre of the vortex the highest compounds are continually being built up and continually being broken down; new material (food, water, oxygen) and energy are brought in at the periphery, and old material and energy (work and heat) thrown out. The principle of the conservation of energy and matter holds good in organised living processes as it does in the inorganic world outside. This is the process we call metabolism, and it is at the base of all the manifestations of life. From the point of view of biological science life is founded on a complex and continuous physico-chemical process of endless duration so long as conditions are favourable; just as a fire will continue to burn so long as fuel is at hand. No one step, no single substance, can be said to be living: the whole chain of substances and reactions, every link of which is essential, constitutes the life-process. A stream of non-living matter with stored-up energy is built up into the living vortex, and again passes out as dead matter, having yielded up the energy necessary for the performance of the various activities of the organism. If more is taken in than is given out it will grow and sub-divide. The complexity of the organism may increase by the formation of subsidiary, more or less interdependent, vortices within it. The perpetual growth and transmission of factors of inheritance, the continuity of the germ-plasm, is but another aspect of the continuity of the metabolic process forming the basis of the continuity of life in evolution.

[5] We might perhaps distinguish the two cases by calling them constant and inconstant characters, or 'natural' and 'acquired,' as is commonly done when describing immunity. It should be meant thereby that one is acquired usually (under normal conditions), the other occasionally (when infection occurs). Error creeps in when the term 'acquired' is opposed to 'non-acquired' or to 'inherited.'

But all environmental stimuli are not external to the organism. Just as the various steps in the metabolic process are dependent on those which preceded them, so when an organism becomes differentiated into parts, when the main process becomes sub-divided into subsidiary ones, these react on each other. What is internal to the whole becomes external to the part. An external stimulus may set up an internal metabolic change, giving rise to a response whose extent and nature depend on the structure of the mechanism and its state when stimulated, that is to say, on the effect of previous responses. Such a response may act as an internal stimulus giving rise to a further response, which may modify the first, and so on. Parts thus become marvellously fitted to set going, inhibit, or regulate each other's action; and thus arises that power of individual adaptation, or self-regulation, so characteristic of living organisms. The processes of temperature regulation, of respiration, of excretion are examples of such delicate self-regulating mechanisms in ourselves. But one of the great advantages thereby gained by organisms is that they can regulate their own growth and ensure their own 'right' development. Whereas the simplest plants and animals are to a great extent, so to speak, at the mercy of their external environment, except in so far as they can move from unfavourable to more favourable surroundings; whereas their characters appear in response to external stimuli which may or may not be present, and over which they have little or no control—the higher organisms (more especially the higher animals), as it were, gradually substitute internal for external stimuli. Food material is provided in the ovum, and the size, structure and time of appearance of various characters are regulated to a great extent by use and by the secretions of various endocrinal glands, the action of which has been so successfully studied, among others, by Sir Sharpey Schaffer in this University. Thus, as is well shown in man, the higher animals acquire considerable independence, and are little affected in their development by minor changes of environment. Inheritance is thus made secure by ensuring that the necessary conditions are always present.

We may seem to have wandered far from our original question; but the answer now appears to be that only those characters can be regularly inherited which depend for their appearance on conditions always fulfilled in the normal environment (external or internal); and those characters will not be regularly inherited which depend on stimuli that may or may not be present. Thus, while the offspring of a dark-skinned race will be dark in whatever climate they are born, those of a fair-skinned race will be born fair, but may be darkened by sun-burn, if they spend their holiday in the open.

Now it will be said, and not without some truth, that all this is mere commonplace admitted by all; but, if so, it is, I think, often ignored or misunderstood in discussions on heredity, more especially in semi-popular writings, and sometimes even in scientific works. However, I quite willingly admit that the real problems Darwin left to be solved by the evolutionist are the nature of the germinal factors themselves, and more especially the origin of the differences between them, the origin of those changes which give rise to mutations.

That these factors[6] must at least be self-propagating substances, subsidiary vortices in the main stream of metabolising living protoplasm, is certain, since they grow and multiply repeatedly, to be distributed to new generations of germ-cells. That they may be relatively constant and remain unaltered for generations seems also certain, since organisms or their parts can continue almost unchanged for untold ages. That they can act independently, can be separately distributed into different germ-cells, and can be re-combined seems likewise to have been proved by the brilliant work of Mendel and his followers. So independent and constant do they appear to be that modern students of heredity tend to treat them as so many beads in a row, as separate particles themselves endowed with all the properties of independent living organisms, the very properties we wish to explain. While not prepared to accept these views without qualification, it seems to me that it can scarcely be doubted that some such units must exist whether in the form of discrete particles or merely of separable substances. But not until these factors have been brought into relation with the general metabolism of the organism, as links in the chain of processes, will the problem of inheritance approach solution. If the theory is to be completed it must attempt to explain how they come to differ, how their orderly behaviour is regulated, in what functional relation they stand to each other, what is the metabolic bond between them. That harmonious processes may be carried out by discrete elements in co-operation is shown in cases of symbiotic combinations such as the lichens, or the green algæ in such animals as Hydra and Convoluta. Here an originally independent organism takes its place and does its work regularly in another organism, and may even be propagated and transmitted from one generation to the next in the germ-cell! Most instructive, also, are the recently studied cases of bacteria and yeasts living regularly in certain special tissues of various species of insects, where they exert a definite influence on the metabolism (see the works of Pierantoni, Buchner, Glaser). These no doubt are mere analogies, but they serve.

In all probability, then, factors of inheritance exist, and the fundamental problem of Biology is how are the factors of an organism changed, or how does it acquire new factors? In spite of its vast importance, it must be confessed that little advance has been made towards the solution of this problem since the time of Darwin, who considered that variation must ultimately be due to the action of the environment. This conclusion is inevitable, since any closed system will reach a state of equilibrium and continue unchanged, unless affected from without. To say that mutations are due to the mixture or reshuffling of pre-existing factors is merely to push the problem a step

[6] Herbert Spencer's 'physiological units,' Darwin's 'pangens,' Weismann's 'determinants,' are all terms denoting factors, but with somewhat different meanings. More recently Professor W. Johannsen (*Elemente der exakten Erblichkeitslehre*, 1909) has proposed the term 'gene' for a factor, 'genotype' for the whole assemblage of factors transmitted by a species, and 'phenotype' for the characters developed from them. This clear system of nomenclature, although much used in America, has not been generally adopted in this country.

farther back, for we must still account for their origin and diversity. The same objection applies to the suggestion that the complex of factors alters by the loss of certain of them. To account for the progressive change in the course of evolution of the factors of inheritance and for the building up of the complex it must be supposed that from time to time new factors have been added; it must further be supposed that new substances have entered into the cycle of metabolism, and have been permanently incorporated as self-propagating ingredients entering into lasting relation with pre-existing factors. We are well aware that living protoplasm contains molecules of large size and extraordinary complexity, and that it may be urged that by their combination in different ways, or by the mere regrouping of the atoms within them, an almost infinite number of changes may result, more than sufficient to account for the mutations which appear But this does not account for the building up of the original complex. If it must be admitted that such a building process once occurred, what right have we to suppose that it ceased at a certain period? We are driven, then, to the conclusion that in the course of evolution new material has been swept from the banks into the stream of germ-plasm.

If one may be allowed to speculate still further, may it not be supposed that factors differ in their stability?—that whereas the more stable are merely bent, so to speak, in this or that direction by the environment, and are capable of returning to their original condition, as a gyroscope may return to its former position when pressure is removed, other less stable factors may be permanently distorted, may have their metabolism permanently altered, may take up new substance from the vortex, without at the same time upsetting the system of delicate adjustments whereby the organism keeps alive? In some such way we imagine factorial changes to be brought about and mutations to result.

Let it not be thought for a moment that this admission that factors are alterable opens the door to a Lamarckian interpretation of evolution! According to the Lamarckian doctrine, at all events in its modern form, a character would be inherited after the removal of the stimulus which called it forth in the parent. Now of course, a response once made, a character once formed, may persist for longer or shorter time according as it is stable or not; but that it should continue to be produced when the conditions necessary for its production are no longer present is unthinkable. It may, however, be said that this is to misrepresent the doctrine, and that what is really meant is that the response may so react on and alter the factor as to render it capable of producing the new character under the old conditions. But is this interpretation any more credible than the first?

Let us return to the possible alteration of factors by the environment. Unfortunately there is little evidence as yet on this point. In the course of breeding experiments the occurrence of mutations has repeatedly been observed, but what led to their appearance seems never to have been so clearly established as to satisfy exacting critics. Quite lately, however, Professor M. F. Guyer, of Wisconsin, has brought forward a most interesting case of the apparent alteration at will of a

factor or set of factors under definite well-controlled conditions.[7] You will remember that if a tissue substance, blood-serum for instance, of one animal be injected into the circulation of another, this second individual will tend to react by producing an anti-body in its blood to antagonise or neutralise the effect of the foreign serum. Now Professor Guyer's ingenious experiments and results may be briefly summarised as follows. By repeatedly injecting a fowl with the substance of the lens of the eye of a rabbit he obtained anti-lens serum. On injecting this 'sensitised' serum into a pregnant female rabbit it was found that, while the mother's eyes remained apparently unaffected, some of her offspring developed defective lenses. The defects varied from a slight abnormality to almost complete disappearance. No defects appeared in untreated controls, no defects appeared with non-sensitised sera. On breeding the defective offspring for many generations these defects were found to be inherited, even to tend to increase and to appear more often. When a defective rabbit is crossed with a normal one the defect seems to behave as a Mendelian recessive character, the first generation having normal eyes and the defect reappearing in the second. Further, Professor Guyer claims to have shown that the defect may be inherited through the male as well as the female parent, and is not due to the direct transmission of anti-lens from mother to embryo *in utero*.

If these remarkable results are verified, it is clear that an environmental stimulus, the anti-lens substance, will have been proved to affect not only the development of the lens in the embryo, but also the corresponding factors in the germ-cells of that embryo; and that it causes, by originating some destructive process, a lasting transmissible effect giving rise to a heritable mutation.

Professor Guyer, however, goes farther, and argues that, since a rabbit can also produce anti-lens when injected with lens substance, and since individual animals can even produce anti-bodies when treated with their own tissues, therefore the products of the tissues of an individual may permanently affect the factors carried by its own germ-cells. Moreover he asks, pointing to the well-known stimulative action of internal secretions (hormones and the like), if destructive bodies can be produced, why not constructive bodies also? And so he would have us adopt a sort of modern version of Darwin's theory of Pangenesis, and a Lamarckian view of evolutionary change.

But surely there is a wide difference between such a poisonous or destructive action as he describes and any constructive process. The latter must entail, as I tried to show above, the drawing of new substances into the metabolic vortex. Internal secretions are themselves but characters, products (perhaps of the nature of ferments) behaving as environmental conditions, not as self-propagating factors, moulding the responses, but not permanently altering the fundamental structure and composition of the factors of inheritance.

Moreover, the early fossil vertebrates had, in fact, lenses neither larger nor smaller on the average than those of the present day. If

[7] *American Naturalist*, vol. lv. 1921; *Jour. of Exper. Zoology*, vol. xxxi. 1920.

destructive anti-lens had been continually produced and had acted, its effect would have been cumulative. A constructive substance must, then, have also been continually produced to counteract it. Such a theory might perhaps be defended; but would it bring us any nearer to the solution of the problem?

The real weakness of the theory is that it does not escape from the fundamental objections we have already put forward as fatal to Lamarckism. If an effect has been produced, either the supposed constructive substance was present from the first, as an ordinary internal environmental condition necessary for the normal development of the character, or it must have been introduced from without by the application of a new stimulus. The same objection does not apply to the destructive effect. No one doubts that if a factor could be destroyed by a hot needle or picked out with fine forceps the effects of the operation would persist throughout subsequent generations.

Nevertheless, these results are of the greatest interest and importance, and, if corroborated, will mark an epoch in the study of heredity, being apparently the first successful attempt to deal experimentally with a particular factor or set of factors in the germ-plasm.

There remains another question we must try to answer before we close, namely, 'What share has the mind taken in evolution?' From the point of view of the biologist, describing and generalising on what he can observe, evolution may be represented as a series of metabolic changes in living matter moulded by the environment. It will naturally be objected that such a description of life and its manifestations as a physico-chemical mechanism takes no account of mind. Surely, it will be said, mind must have affected the course of evolution, and may indeed be considered as the most important factor in the process. Now, without in the least wishing to deny the importance of the mind, I would maintain that there is no justification for the belief that it has acted or could act as something guiding or interfering with the course of metabolism. This is not the place to enter into a philosophical discussion on the ultimate nature of our experience and its contents, nor would I be competent to do so; nevertheless, a scientific explanation of evolution cannot ignore the problem of mind if it is to satisfy the average man.

Let me put the matter as briefly as possible at the risk of seeming somewhat dogmatic. It will be admitted that all the manifestations of living organisms depend, as mentioned above, on series of physico-chemical changes continuing without break, each step determining that which follows; also that the so-called general laws of physics and of chemistry hold good in living processes. Since, so far as living processes are known and understood, they can be fully explained in accordance with these laws, there is no need and no justification for calling in the help of any special vital force or other directive influence to account for them. Such crude vitalistic theories are now discredited, but tend to return in a more subtle form as the doctrine of the interaction of body and mind, of the influence of the mind on the activities of the body. But, try as we may, we cannot conceive how a physical process can be interrupted or supplemented by non-physical agencies.

Rather do we believe that to the continuous physico-chemical series of events there corresponds a continuous series of mental events inevitably connected with it; that the two series are but partial views or abstractions, two aspects of some more complete whole, the one seen from without, the other from within, the one observed, the other felt. One is capable of being described in scientific language as a consistent series of events in an outside world, the other is ascertained by introspection, and is describable as a series of mental events in psychical terms. There is no possibility of the one affecting or controlling the other, since they are not independent of each other. Indissolubly connected, any change in the one is necessarily accompanied by a corresponding change in the other. The mind is not a product of metabolism as materialism would imply, still less an epiphenomenon or meaningless by-product as some have held. I am well aware that the view just put forward is rejected by many philosophers, nevertheless it seems to me to be the best and indeed the only working hypothesis the biologist can use in the present state of knowledge. The student of biology, however, is not concerned with the building up of systems of philosophy, though he should realise that the mental series of events lies outside the sphere of natural science.

The question, then, which is the more important in evolution, the mental or the physical series, has no meaning, since one cannot happen without the other. The two have evolved together *pari passu*. We know of no mind apart from body, and have no right to assume that metabolic processes can occur without corresponding mental processes, however simple they may be.

Simple response to stimulus is the basis of all behaviour. Responses may be linked together in chains, each acting as a stimulus to start the next; they can be modified by other simultaneous responses, or by the effects left behind by previous responses, and so may be built up into the most complicated behaviour. But, owing to our very incomplete knowledge of the physico-chemical events concerned, we constantly, when describing the behaviour of living organisms, pass, so to speak, from the physical to the mental series, filling up the gaps in our knowledge of the one from the other. We thus complete our description of behaviour in terms of mental processes we know only in ourselves (such as feeling, emotion, will) but infer from external evidence to take place in other animals.

In describing a simple reflex action, for instance, the physico-chemical chain of events may appear to be so completely known that the corresponding mental events are usually not mentioned at all, their existence may even be denied. On the contrary, when describing complex behaviour when impulses from external or internal stimuli modify each other before the final result is translated into action, it is the intervening physico-chemical processes which are unknown and perhaps ignored, and the action is said to be voluntary or prompted by emotion or the will.

The point I wish to make, however, is that the actions and behaviour of organisms are responses, are characters in the sense described in the earlier part of this address. They are inherited, they

vary, they are selected, and evolve like other characters. The distinction so often drawn by psychologists between instinctive behaviour said to be inherited and intelligent behaviour said to be acquired is as misleading and as little justified in this case as in that of structural characters. Time will not allow me to develop this point of view, but I will only mention that instinctive behaviour is carried out by a mechanism developed under the influence of stimuli, chiefly internal, which are constantly present in the normal environmental conditions, while intelligent behaviour depends on responses called forth by stimuli which may or may not be present. Hence, the former is, but the latter may or may not be inherited. As in other cases, the distinction lies in the factors and conditions which produce the results. Instinctive and intelligent behaviour are usually, perhaps always, combined, and one is not more primitive or lower than the other.

It would be a mistake to think that these problems concerning factors and environment, heredity and evolution, are merely matters of academic interest. Knowledge is power, and in the long run it is always the most abstruse researches that yield the most practical results. Already, in the effort to keep up and increase our supply of food, in the constant fight against disease, in education, and in the progress of civilisation generally, we are beginning to appreciate the value of knowledge pursued for its own sake. Could we acquire the power to control and alter at will the factors of inheritance in domesticated animals and plants, and even in man himself, such vast results might be achieved that the past triumphs of the science would fade into insignificance.

Zoology is not merely a descriptive and observational science, it is also an experimental science. For its proper study and the practical training of students and teachers alike, well-equipped modern laboratories are necessary. Moreover, if there is to be a useful and progressive school contributing to the advance of the science, ample means must be given for research in all its branches. Life doubtless arose in the sea, and in the attempt to solve most of the great problems of biology the greatest advances have generally been made by the study of the lower marine organisms. It would be a thousand pities, therefore, if Edinburgh did not avail itself of its fortunate position to offer to the student opportunities for the practical study of marine zoology.

In his autobiography, Darwin complains of the lack of facilities for practical work—the same need is felt at the present time. He would doubtless have been gratified to see the provision made since his day and the excellent use to which it has been put; but what seems adequate to one generation becomes insufficient for the next. We earnestly hope that any appeal that may be made for funds to improve this Department of Zoology may meet with the generous response it certainly deserves.

APPLIED GEOGRAPHY.

ADDRESS BY

D. G. HOGARTH, M.A., D.LITT., C.M.G.,

PRESIDENT OF THE SECTION.

THE term which I have taken for the title of my address has been in use for some years as a general designation of lendings or borrowings of geographical results, whether by a geographer who applies the material of his own science to another, or by a geologist or a meteorologist, or again an ethnologist or historian, who borrows of the geographer. Whether Geography makes the loan of her own motion or not, the interest in view, as it seems to me, is primarily that, not of Geography, but of another science or study. The open question whether that interest will be served better if the actual application be made by the geographer or by the other scientist or student does not concern us now.

Such applications are of the highest interest and value as studies, and, still more, as means of education. As studies, not merely are they links between sciences, but they tend to become new subjects of research, and to develop with time into independent sciences. As means of education they are used more generally, and prove themselves of higher potency than the pure sciences from which or to which, respectively, the loans are effected. But, in my view, Geography, thus applied, passes, in the process of application, into a foreign province and under another control. It is most proper, as well as most profitable, for a geographer to work in that foreign field; but, while he stays in it, he is, in military parlance, seconded.

Logical as this view may appear, and often as, in fact, it has been stated or implied by others (for example, by one at least of my predecessors in this chair, Sir Charles Close, who delivered his presidential address to the section at the Portsmouth Meeting in 1911), it does not square with some conceptions of Geography put forward by high authorities of recent years. These represent differently the status of some of the studies, into which, as I maintain, Geography enters as a subordinate and secondary element. In particular, there is a school, represented in this country and more strongly in America, which claims for Geography what, in my view, is an historical or ethnological or even psychological study, using geographical data towards the solution of problems in its own field; and some even consider this not merely a function of true Geography, but its principal function now and for the future. Their 'New Geography' is and is to be the study of 'human response to land-forms.' This is an extreme American statement; but the same idea is instinct in such utterances, more sober and guarded, as that of a great geographer, Dr. H. R. Mill, to the effect that the *ultimate* problem of Geography is 'the demonstrative and quantitative proof of the control exercised by the Earth's crust on

the mental processes of its inhabitants.' Dr. Mill is too profound a man of science not to guard himself, by that saving word 'ultimate,' from such retorts as Professor Ellsworth Huntington, of Yale, has offered to the extreme American statement. If, the latter argued, geography is actually the study of the human response to land-forms, then, as a science it is in its infancy, or, rather, it has returned to a second childhood; for it has hardly begun to collect exact data to this particular end, or to treat them statistically, or to apply to them the methods of isolation that exact science demands. In this country geographers are less inclined to interpret 'New Geography' on such revolutionary lines; but one suspects a tendency towards the American view in both their principles and their practice—in their choice of lines of inquiry or research and their choice of subjects for education. The concentration on Man, which characterises geographical teaching in the University of London, and the almost exclusive attention paid to Economic Geography in the geographical curricula of some other British Universities make in that direction. In educational practice, this bias does good, rather than harm, if the geographer bears in mind that Geography proper has only one function to perform in regard to Man—namely, to investigate, account for, and state his distribution over terrestrial space—and that this function cannot be performed to any good purpose except upon a basis of Physical Geography—that is, on knowledge of the disposition and relation of the Earth's physical features, so far as ascertained to date. To deal with the effect of Man's distribution on his mental processes or political and economic action is to deal with him geographically indeed, but by applications of Geography to Psychology, to History, to Sociology, to Ethnology, to Economics, for the ends of these sciences; though the interests of Geography may be, and often are, well served in the process by reflection of light on its own problems of distribution. If in instruction, as distinct from research, the geographer, realising that, when he introduces these subjects to his pupils, he will be teaching them not Geography, but another science with the help of Geography, insists on their having been grounded previously or elsewhere in what he is to apply—namely, the facts of physical Distribution—all will be well. The application will be a sound step forward in education, more potent perhaps for training general intelligence than the teaching of pure Geography at the earlier stage, because making a wider and more compelling appeal to imaginative interest and pointing the adolescent mind to a more complicated field of thought. But if Geography is applied to instruction in other sciences without the recipients having learned what it is in itself, then all will be wrong. The teacher will talk a language not understood, and the value of what he is applying cannot be appreciated by the pupils.

If I leave this argument there for the moment, it is with the intention of returning to it before I end to-day. It goes to the root, as it seems to me, of the unsatisfactory nature of much geographical instruction given at present in our islands. The actual policy of the English Board of Education seems to contemplate that Geography should be taught to secondary students only in connection with History. If

this policy were realised in instructional practice by encouragement or compulsion of secondary students to undergo courses of Geography proper, with a view to promotion subsequently to classes in Historical Geography (*i.e.*, if History be treated geographically by application of another science previously studied), it would be sound. But I gather from Sir Halford Mackinder's recent report that such is not the practice. Courses in Geography proper are not encouraged during the secondary period of education at all. Encouragement ceases with the primary period, at an age before which only the most elementary instruction in such a science can be assimilated—when, indeed, not much more can be expected of pupils than the memorising of those summary diagrammatic expressions of geographical results, which are maps. How these results have been arrived at, what sort of causes account for physical Distribution, how multifarious are its facts and features which maps cannot express even on the minutest scale—these things must be instilled into minds more robust than those of children under fourteen; and until some adequate idea of them has been imbibed it is little use to teach History geographically. So, at least, this matter seems to me.

It will be patent enough by now that I am maintaining Geography proper to be the study of the spatial Distribution of all physical features on the surface of this Earth. My view is, of course, neither novel nor rare. Almost all who of late years have discussed the scope of Geography have agreed that Distribution is of its essence. Among the most recent exponents of that view have been two Directors of the Oxford School, Sir Halford Mackinder and Professor Herbertson. When, however, I add that the study of Distribution, rightly understood, is the whole essential function of Geography, I part company from the theory of some of my predecessors and contemporaries, and the practice of more. But our divergence will be found to be not serious; for not only do I mean a great deal by the study of Distribution—quite enough for the function of any one science!—but I claim for Geography to the exclusion of any other science all study of spatial Distribution on the Earth's surface. This study has been its well recognised function ever since a science of that name has come to be restricted to the features of the terrestrial surface—that is, ever since 'Geography' in the eighteenth century had to abandon to its child Geology the study of what lies below that surface even as earlier it had abandoned the study of the firmament to an elder child, Astronomy. Though Geography has borne other children since, who have grown to independent scientific life, none of these has robbed her of that one immemorial function. On the contrary, they call upon her to exercise it still on their behalf.

Let no one suppose that I mean by this study and this function merely what Professor Herbertson so indignantly repudiated for an adequate content of his Science—Physiography *plus* descriptive Topography. Geography includes these things, of course, but she embraces also all investigation both of the actual Distribution of the Earth's superficial features and of the causes of the Distribution, the last a profound and intricate subject towards the solution of which she has to summon assistance from many other sciences and studies. She includes, further,

in her field, for the accurate statement of actual Distribution, all the processes of Survey—a highly specialised function to the due performance of which other sciences again lend indispensable aid; and, also, for the diagrammatic presentation of synthetised results for practical use, the equally highly specialised processes of Cartography. That seems to me an ample field, with more than sufficient variety of expert functions, for any one Science. And I have not taken into account either the part Geography has to play in aiding other sciences, as they aid her, by application of her data, or, again, certain investigations of terrestrial phenomena, at present incumbent upon her, because special sciences to deal with them have not yet been developed—or, at least, fully developed—although their ultimate growth to independence can be foreseen or has already gone far. Such, for the moment, are Geodetic investigations, in this country at any rate. In Germany, I understand, Geodesy has attained already the status of a distinct specialism. Here the child has hardly separate existence. But beyond a doubt it will part from its parent, even as Oceanography has parted. Indeed some day, in a future far too distant to be foreseen now, many, or most, of the investigations which now occupy the chief attention of geographical researchers may cease to be necessary. A time must come when the actual distribution of all phenomena on the Earth's surface will have been ascertained, and all the relief upon it and every superficial feature which Cartography can possibly express in its diagrammatic way will have been set out finally on the map. That moment, however, will not be the end of Geography as a science, for there will still remain the investigation of the causes of Distribution, the scientific statement of its facts, and the application of these to other sciences. Let us not, however, worry about any ultimate restriction of the functions of our Science. The discovery and correlation of all the facts of geographical Distribution and their final presentation in diagrammatic form are not much more imminent than the exhaustion of the material of any other science!

In the meantime, for a wholly indeterminate interval, let us see to it that all means of investigating the phenomena of spatial Distribution on the Earth be promoted, without discouragement of this or that tentative means as unscientific. The exploration of the terrestrial surface should be appreciated as a process of many necessary stages graduated from ignorance up to perfect knowledge. It is to the credit of the Royal Geographical Society that it has always encouraged tentative, and, if you like, unscientific first efforts of exploration, especially in parts of the world where, if every prospect pleases, Man is very vile. Unscientific explorations are often the only possible means to the beginning of knowledge. Where an ordinary compass cannot be used except at instant risk of death it is worth while to push in a succession of explorers unequipped with any scientific knowledge or apparatus at all, not merely to gain what few geographical data untrained eyes may see and uneducated memories retain, but to open a road on which ultimately a scientific explorer may hope to pass and work, because the local population has grown, by intercourse with his unscientific precursors, less hostile and more indifferent to his prying activities. There seems

to me now and then to be too much criticism of Columbus. If he thought America was India he had none the less found America.

I have claimed for the geographer's proper field the study of the causation of Distribution. I am aware that this claim has been, and is, denied to Geography by some students of the sciences which he necessarily calls to his help. But if a Science is to be denied access to the fields of other sciences except it take service under them, what science shall be saved? I admit, however, that some disputes can hardly be avoided, where respective boundaries are not yet well delimited. Better delimitation is called for in the interest of Geography, because lack of definition, causing doubts and questions about her scope, confuses the distinction between the Science and its Application. The doubts are not really symptoms of anything wrong with Geography, but, since they may suggest to the popular mind that in fact something is wrong, they can be causes of disease. Their constant genesis is to be found in the history of a Science, whose scope has not always been the same, but has contracted during the course of ages in certain directions while expanding in others. If, in the third century B.C., Eratosthenes had been asked what he meant by Geography, he would have replied, the science of all the physical environment of Man whether above, upon, or below the surface of the Earth, as well as of Man himself as a physical entity. He would have claimed for its field what lies between the farthest star and the heart of our globe, and the nature and relation of everything composing the universe. Geography, in fact, was then not only the whole of Natural Science, as we understand the term, but also everything to which another term, Ethnology, might now be stretched at its very widest.

Look forward now across two thousand years to the end of the eighteenth century A.D. Geography has long become a Mother. She has conceived and borne Astronomy, Chemistry, Botany, Zoology, and many more children, of whom about the youngest is Geology. They have all existences separate from hers and stand on their own feet, but they preserve a filial connection with her and depend still on their Mother Science for a certain common service, while taking off her hands other services she once performed. Restricting the scope of her activities, they have set her free to develop new ones. In doing this she will conceive again and again and bear yet other children during the century to follow—Meteorology, Climatology, Oceanography, Ethnology, Anthropology, and more. Again, and still more narrowly, this new brood will limit the Mother's scope; but ever and ever fecund, she will find fresh activities in the vast field of Earth knowledge, and once and again conceive anew. The latest child that she has borne and seen stand erect is, as I have said, Geodesy; and she has not done with conceiving.

Ever losing sections of her original field and functions, ever adding new sections to them, Geography can hardly help suggesting doubts to others and even to herself. There must always be a certain indefiniteness about a field on whose edges fresh specialisms are for ever developing towards a point at which they will break away to grow alone into new sciences. The Mother holds on awhile to the

child, sharing its activities, loth to let go, perhaps even a little jealous of its growing independence. It has not been easy to say at any given moment where Geography's functions have ended and those of, say, Geology or Ethnology have begun. Moreover, it is inevitably asked about this fissiparous science from which function after function has detached itself to lead life apart—what, if the process continues, as it shows every sign of doing, will be left to Geography later or sooner? Will it not be split up among divers specialisms, and become in time a venerable memory? It is a natural, perhaps a necessary, question. But what is wholly unnecessary is that any answer should be returned which implies a doubt that Geography has a field of research and study essentially hers yesterday, to-day, and to-morrow; still less which implies any suspicion that, because of her constant parturition of specialisms Geography is, or is likely in any future that can be foreseen, to be moribund.

Since Geography, as I understand it, is a necessary factor in the study of all sciences, and must be applied to all if their students are to apprehend rightly the distribution of their own material, it is a necessary element in all education. Unless, on the one hand, its proper study be supported by such means as the State, the Universities, and the great scientific Societies control, and, on the other, its application to the instruction of youth be encouraged by the same bodies, the general scientific standard in these islands will suffer; our system of education will lack an instrument of the highest utility for both the inculcation of indispensable knowledge and the training of adolescent intelligence; and a vicious circle will be set up, trained teachers being lacking in quantity and quality to train pupils to a high enough standard to produce out of their number sufficient trained teachers to carry on the torch.

The present policy of the English Board of Education, as expressed in its practice, encourages a four-years' break in the geographical training of the young, the break occurring between the ages of fourteen and eighteen, the best years of adolescent receptivity. If students are to be strangers to specifically geographical instruction during all that period, any geographical bent given to their minds before the age of fourteen is more than likely to have disappeared by the time they come to eighteen years. The habit of thinking geographically—that is, of considering group distribution—cannot have been formed; and the students, not having learned the real nature of the science applied, will not possess the groundwork necessary for the apprehension of the higher applications of Geography. Moreover, as Sir Halford Mackinder has rightly argued, an inevitable conséquence of this policy is that the chief prizes and awards offered at the end of school-time are not to be gained by proficiency in Geography. Therefore, few students are likely to enter the University with direct encouragement to resume a subject dropped long before, at the end of the primary period of their education.

It is not, of course, the business of schools, primary and secondary, to train specialists. Therefore one does not ask that pure

geographical science should have more than a small share of the compulsory curriculum. Only, that it have some share. If this is assured, then its applications, which on account of their highly educative influence deserve an equally compulsory but larger place in the curriculum, can be used to full advantage. The meaning and value of the geographical ingredient in mixed studies will stand good chance of being understood, and of exciting the lively interest of young students. In any case, only so will the Universities be likely to receive year by year students sufficiently grounded to make good use of higher geographical courses, and well enough disposed to Geography to pursue it as a higher study, and become in their turn competent teachers.

The obligation upon the Universities is the same in kind, but qualitatively greater. They have to provide not only the highest teaching, both in the pure science and its applications, but also such encouragements as will induce students of capacity to devote their period of residence to this subject. The first part of this obligatory provision has been recognised and met in varying degrees by nearly all British Universities during the past quarter of a century. A valuable report compiled recently by that veteran champion, Sir John Keltie, shows that, in regard to Geography, endowment of professorial chairs, allocations of stipends to Readers, Lecturers, and Tutors, supply of apparatus for research and instruction and organisation of 'Honour' examinations, have made remarkable progress in our University world as a whole. But no single British University has yet provided all that is requisite or desired. Oxford and Cambridge, which have well-equipped geographical laboratories, still lack professorial chairs. Liverpool, maintaining a well-staffed Department of Geography, and London, which, between University College and the School of Economics, provides all the staff and apparatus required for teaching, have endowed Chairs; but they direct the attention of the holders to applications of Geography rather than to the pure Science. So do also the University of Manchester and the University College of Wales, both of which maintain geographical Professors.

All the Universities, with but one or two exceptions, examine in the subject to a high standard, that set by Cambridge being perhaps the highest over the whole field of properly geographical study. This latter University, also, alone (if I am not mistaken), has met the second part of her obligation to Geography by the organisation of an Honours course of instruction and classified examination, which, if pursued throughout a student's residence, is sufficient in itself to secure graduation. At Cambridge, therefore, Geography may be said to stand on a par with any other self-contained Final Subject. Neither in London nor in Manchester (I am not quite sure about Liverpool, but believe its case to be the same) is Geography, in and by itself, all sufficient yet to secure graduation, though at each of these Universities it counts strongly in the Baccalaureate Honours course. Oxford offers distinctly less encouragement at present than any of the Universities just mentioned. Her teaching and her examination standard are as advanced as the best of theirs, and the highest award which she

gives for proficiency in Geography, her Diploma 'with distinction' counts towards the B.A. degree in at least as great a proportion as at any other University except Cambridge—it counts, in fact, as two-thirds of the whole qualification; but—and here's the rub!—the balance has to be made up by proficiency in some other subject up to a pass, not an Honours standard. Therefore the resultant degree does not stand before the world as one taken in Honours; and, although some candidates are notified as distinguished and some not in the geographical part of her examinations, the distinction is not advertised in the form to which the public is accustomed—namely, an Honours list divided into classes. The net result is that an Oxford diploma, however brilliantly won, commands less recognition in the labour market than would a class in an Honour School or Tripos. It should, however, be mentioned—though an infrequent occurrence, not advertised by a class list, makes little impression on public opinion—that special geographical research, embodied in a thesis, can qualify at Oxford for higher degrees than the B.A.—viz., for the B.Litt. and B.Sc.—without the support of other subjects.

The reason of this equivocal status of Geography at Oxford is simply that, so far as the actual Faculties which control the courses for ordinary graduation are concerned, Geography is, in fact, an equivocal subject. No one Faculty feels that it can deal with the whole of it. The Arts Faculties will not accept responsibility for the elements of Natural and Mathematical Science which enter into its study and teaching—for example, into the investigation of the causes of Distribution, into the processes of Surveying, into Cartography, and into many other of its functions. Moreover, the traditional Oxford requirement of a literary basis for Arts studies is hard, if not impossible, to satisfy in Geography. The Faculty of Natural Science, on the other hand, is equally loth to be responsible for a subject which admits so much of the Arts element, especially into those applications of its data which enter most often into the instructional curriculum of adolescents—for example, its applications to History and to Ethnology.

At this moment, then, there is an *impasse* at Oxford similar to that (it is caused by the same reason) which prevents the election of a Geographer, as such, either to the Royal Society on the one hand, or to the British Academy on the other. But ways out can be found if there be good will towards Geography, and such general recognition of the necessity of bringing it into closer relation with the established studies, as was implied by the examiners in the Oxford School of Literae Humaniores last year, when, in an official notice, they expressed their sense of a lack of it in the historical work with which they had to deal. Faculties are comparatively modern organisations at Oxford as at Cambridge for the control of teaching and examining. Before them existed Boards of Studies, appropriated to narrower subjects; and, indeed, such Boards have been constituted since Faculties became the rule and side by side with them. The Board, which at the first controlled at Oxford the Final Honour School of English, is an example and a valid precedent

Cambridge has found it possible to organise a mixed Board of Studies to manage a Final School of Geography, the Board being composed of representatives of both the Arts subjects and the Natural and Mathematical Sciences; and this acts apparently to the general satisfaction even in the absence of a Professor of the special subject, for whose teaching and testing it was formed. Why, then, should Oxford not do likewise? If Cambridge has not waited for the endowment of a Professorial Chair in Geography, need Oxford wait? I am well aware that, when at the latter University the School of English came into existence, there were already two Chairs appropriated to its subject; and I grant that Oxford will not have the very best of all guarantees that a high standard will be maintained in the instructional courses and the examinations in Geography, until there is a Professor *ad hoc*. But guarantees sufficient for all practical purposes she could obtain to-morrow by composing a Board out of her existing teachers of Geography and kindred sciences.

For the last time, then, let me rehearse the too familiar 'vicious circle.' The supply of good students depends on a supply of good teachers; the supply of good teachers depends on a supply of good students. If either supply fails, it is not Geography alone, but all sciences and studies that will be damnified; for all require the best of the help she can give in proportion as her science grows and improves. History will be able to call but indifferent Geography to her assistance, if this science has been understaffed and discouraged by official reluctance to allow it a place of its own in the sun. Is there not still some such reluctance on the part of the Board of Education, of some of our Universities, and of the Civil Service Commissioners?

THE PRINCIPLES BY WHICH WAGES ARE DETERMINED.

ADDRESS BY

W. L. HICHENS,

PRESIDENT OF THE SECTION.

I HAVE chosen as the subject of my address 'The Principles by which Wages are determined' because I think the most burning question in the industrial world to-day is the proper apportionment of the proceeds of industry between labour and capital. A strong feeling exists in the minds of many that the share of capital is too large and that labour is, in consequence, underpaid. There are those, of course, who hold with Mr. Tawney that capital is functionless and therefore entitled to no reward. It is not my intention to examine the grounds for this statement, for no one who has any experience of business can fail to recognise that under the existing organisation of business capital has a very definite function—that it is indeed essential to any industrial organisation. If there is anyone in this room who has had dealings in the City for the purpose of raising a loan he will feel acutely—not merely the unpleasant consequences that would have awaited him had none of this functionless capital been placed at his disposal—but also the fact that the capitalist has a very definite idea of the importance of his own function. The capitalist would, indeed, automatically cease to exist if he were not needed and fulfilled no function, and the fact that every industrialist is obliged to go to him—often on bended knee—is sufficient answer to the proposition that he performs no useful service. Acquisitive he may be, but he only acquires wealth because he supplies something for which there is a real need. Capital must be paid for just as much as any other commodity, and if any given industry is unable to pay sufficient to attract it, that industry must inevitably languish and ultimately perish.

Many of our industrial troubles to-day arise from the fact that people concentrate on one aspect of the industrial problem only and refuse to consider it as a whole. They are so intent on the rights of labour or of capital that they overlook the fact that each is necessary to the other, and that neither can exist in isolation from the other. It is clearly important, therefore, that both capital and labour should understand, and, what is more, sympathise with, each other's point of view. And if I may venture a criticism it is that the capitalist has usually an intellectual grasp of the point of view of labour, but fails to bring a sympathetic understanding to bear on its aspirations. Labour, on the other hand, apart from some of the leaders whose opinions are in consequence suspect, neither understands nor sympathises with the capitalist standpoint. This is a real misfortune, for the two are indissolubly bound together, and no progress is possible so long as they

quarrel and pull in different directions. Clubs for the discussion of economic questions should be started all over the country, and every section of opinion should be represented in order that problems may be considered from every point of view.

The wage problem is in essentials simple to grasp; it is the problem of the division of the proceeds of industry between labour and capital. How are we to ensure that neither the capitalist nor the worker gets too large a share of the proceeds of industry? How are we to provide that one class of labour does not get too much in relation to another? How are we to secure that the consumer is not robbed by the exaction of too heavy a toll for services rendered? But if the problem is easy to state the solution is by no means simple. Some people would cut the Gordian knot by referring every dispute as it arises to compulsory arbitration. But there are certain difficulties which must be overcome before arbitration can be successfully applied. In the first place, the principle of arbitration must be generally accepted by both sides. You cannot compel large bodies of men to work for a given wage, for there is no penalty that can be enforced against them if they refuse. Nor can you force employers on a large scale to pay a prescribed wage if they prefer to close their factories. The right to strike and the right to lock out must always lurk in the background, and nothing can prevent the exercise of both if enough men feel sufficiently strongly on one side or the other. The time may come when public opinion will recognise so acutely the evils of the strike and the lock-out that both sides will be prepared to accept the principle of arbitration. It is not perhaps too much to hope that one day the principles of reason and justice will triumph over the prevailing theory that might is right and that the only effective criterion of justice is what a man is strong enough to take and to hold. But that day has not yet dawned, and any scheme of compulsory arbitration would, under present conditions, be foredoomed to failure. Meanwhile, an important step in the right direction has already been taken. The Government has been given powers to institute an inquiry into any trade dispute and to summon witnesses. Hence, although the decision of any such court of inquiry will not be binding on the two parties, yet the proceedings can be published and the public will be enabled to pass judgment on the actual facts. The development of these inquiries will be watched with great interest, and there are strong grounds for hoping that they will exercise an effective influence on the side of peace. It is important to notice that these inquiries will only be held after the two parties have met and failed to reach agreement. For by far the best method of settling a dispute is that the representatives of both sides should meet and negotiate with the object of finding some solution that is mutually satisfactory. It is only in the last resort, when all other means have failed, that recourse should be had to a higher authority. I must add that it is, in my opinion, regrettable that arbitration is not voluntarily accepted by both sides more often. There is a tendency at present for certain groups of employers to refuse arbitration, although the representatives of the Trade Unions concerned are prepared to do so, and I feel that this is a short-sighted policy. It must be confessed also that the feeling amongst employers

that the rank and file of labour would refuse to be bound by an unfavourable arbitration award, even if their leaders had agreed to accept it, is not without foundation.

A second objection to arbitration that has to be met is that the arbitrators must command the confidence of both sides. For arbitration, in the sense of an award made by Government nominees, has long ago been tried and found wanting. Attempts have been made in times past to regulate wages and conditions of work either by Acts of Parliament or by particular orders of the justices of the peace, but, on the whole, the results have been bad, and the well-known criticisms of Adam Smith appear to have been justified. For the laws were made and administered by the employing classes and took little account of the aims and aspirations of the workers. They were essentially conservative in character and aimed rather at preserving the ancient privileges of the ruling classes than at developing the liberties of the ruled. Fortunately the growth of the labour movement has now made it possible to secure both a fair hearing and adequate representation for working-class interests. One result of the development of Trade Unionism has been to create a body of highly trained experts who can be relied on to do full justice to the cause of those whom they have been chosen to represent. The difficulty to-day is that the Trade Union leader, whose education and training have given him a wider grasp of economic problems than is possessed by his constituents, is often not treated with the confidence that he deserves and is not allowed that freedom and power to settle which is essential to the success of all negotiations.

A third objection to arbitration, which leads up to the subject with which I am to deal more particularly to-day, is that there are at present no generally accepted principles governing industrial problems which the arbitrator has to interpret, and yet the success or failure of arbitration as a method of settling industrial disputes depends ultimately on whether there are certain clear principles which the great majority are prepared to accept as just and reasonable. The function of an arbitrator is to interpret and apply accepted principles just as that of a judge is to interpret the principles embodied in the laws. It is not his business to lay down principles, and if he attempts to do so he will probably fail. That is why arbitration has so often miscarried and why the Hague tribunal was foredoomed to failure. For the disputes between nations are not as a rule differences as to the interpretation of a principle; they arise from a conflict of principles. Hence the danger that arbitrators will base their verdict on the wording of treaties and agreements, on precedent and tradition, and serve merely to protect the *status quo*. This is a very real danger in industrial questions, for the industrial machine is extremely sensitive and complex, and needs continually to be adjusted to an ever-changing environment. What is good for to-day will perhaps be wholly unsuited for to-morrow, and no worse fate can befall industry than that it should be fast bound in the tyranny of precedent. Another danger of special application to wages disputes is that, in the absence of real principles, an arbitrator may simply split the difference between the contentions of the disputants,

and there is a widespread feeling that, in the past, wages awards have largely been made by this rule-of-thumb method. It is of the first importance, therefore, that clear and well-recognised principles should be established, and in considering the question one would naturally expect to find guidance in the laws of those States which have adopted compulsory arbitration for wages disputes. Unfortunately they have shirked the difficulty, and left it to the arbitrator to make and interpret his own code of rules. According to some Australian Acts reasonable wages are defined as 'the average prices of payment paid by reputable employers to employees of average capacity.' But the 'reputable employers' clause has proved a broken reed, and embodies no principle of practical value. For, in the first place, there are industries in which a standard wage is paid by *all* employers so that in the event of a dispute there are either no reputable or else no disreputable employers—whichever you please. In the second place, even if there are certain employers in an industry who pay higher wages than others, it does not follow either that the employers who pay less are not reputable or that the higher-paid employees are of average capacity. The probability is that they are not—that they are above the average. The justification for paying higher wages is that you get the pick of the basket by so doing. And efficiency is so important that it is worth the while of any given firm to adopt this course if it can be sure that others will not follow suit. If, unfortunately for it, they do, it no longer gets the pick, and the game is spoiled. Henry Ford is only able to pay higher wages than his rivals because this policy enables him to adopt the strictest tests of efficiency.

The weakness of this clause has led the Australian Commonwealth to adopt another principle for the guidance of arbitrators, namely, that the conditions as to the remuneration of labour are to be such as the President of the Commonwealth Court of Conciliation and Arbitration shall declare to be fair and reasonable. That is not a very illuminating or helpful principle, and Mr. Higgins, the President of the Court, has complained very bitterly that the Legislature has left to him what it ought to have done itself by defining what is meant by 'fair and reasonable.' Everyone, even the disreputable employer, will agree that wages must be fair and reasonable, but with this meaningless proposition our unanimity comes to an abrupt end, for we find the most divergent views as to what constitutes fairness or reasonableness. One school—and a powerful one, too—holds that a fair wage is one that is determined by the higgling of the market, that it is established by the law of supply and demand. 'The money rate of wages,' says Walter Bagehot, 'is a case of supply and demand—that is, it is determined by the amount of money which the owners of it wish to expend in labour, by the eagerness with which they want that labour, by the amount of labour in the market which wishes to sell itself for money, and by the eagerness with which the labourers desire that money.' No doubt in a perfect world, and if everyone were a perfectly free agent, the law of supply and demand might safely be left to take its own course. And even in the imperfect world in which we live it has its value as a criterion in the determination of wages, and must always be regarded

as one of the decisive factors, though not by any means the only one. It is true that the law may be harsh and inhuman in its operation when applied by harsh and inhuman men, but it has often proved of more advantage to the workers themselves than the solicitude of Parliament or its agents. There is a familiar ring about the history of a wages award in the London tailoring trade made by the justices of the peace in 1771. It can be paralleled in any country in any age. The wages of London tailors were settled at 2*s*. 6*d*. a day, 'but many master tailors gave some of their men 3*s*. a day; they paid the 15*s*. at the end of the week openly, and then put 3*s*. more for a man in some place where he knew where to find it. And if this money was not laid up for him on the Saturday night the master might be certain not to see his face on the Monday morning.'

But it must be remembered that in very many cases we are not free economic agents, and the open competition which is essential to the successful functioning of the law of supply and demand is conspicuously absent. It is absent, for example, in the case of a general coal strike or a railway strike, where the whole community is threatened with disaster. It is absent, too, if the employers in any big industry threaten a lock-out as the alternative to a reduction in wages, because there is no reasonable chance for the men to find other employment, and starvation may stare them in the face. In such cases the law of supply and demand should not be allowed to decide the issue, for the economic wage would be either too high or too low from the standpoint of what is fair and reasonable. State intervention therefore becomes necessary. But the law of supply and demand always has been one of the chief factors in determining the price of labour, and will continue to be so as long as the existing industrial system lasts. For it is merely another way of stating that a free exchange of services, as of commodities, is the foundation of all trade. Indeed, no other practicable method has ever been devised for determining the relative value of certain services. When a new industry is started, for example, it is necessary to attract workers from those already in existence, and this can only be done by offering higher wages or better conditions. Similarly, if there is a shortage of workers in any established industry owing to increased prosperity, the *personnel* must be drawn from outside by the offer of greater inducements unless the shortage can be made good from the ranks of the unemployed. Again, if there is a general expansion of industry throughout the country, accompanied as it must be by a general increase of wealth, the greater demand for workers will cause wages to rise. Indeed, as Adam Smith has pointed out, it is in a progressive society, in which the demand for labour continually rises, that wages are highest. 'It is not the actual greatness of national wealth,' he says truly—and we shall do well to take his words to heart in these days—'but its continued increase which occasions a rise in the wages of labour.' In a stationary or declining society wages are bound to fall.

But, important though the part played by the law of supply and demand is in determining wages, there is another equally important

principle which governs them—namely, that all men must be paid a living wage. The former is easy to understand and works automatically, though not always satisfactorily. It is important to remember, however, that if the law of supply and demand works badly the fault lies not with political economy but with ourselves. The fact that wages postulate a willing buyer and a willing seller of labour does not justify the employer in driving the hardest bargain he can. The interpretation of this law must be consistent with the higher moral law of our duty towards our neighbour, and the many shortcomings in our industrial life may, in my opinion, be attributed entirely to the fact that we have failed to apply the moral law. It is not the system which is wrong, but those who work it—employers, employed, and consumers alike; it is the hearts of men that must be changed, not the forms of industrial organisation, if we are to cure industrial unrest.

But, if the law of supply and demand is easily intelligible, the principle of the living wage has given rise to many controversies. It is obvious that a man must be paid at least enough to keep body and soul together, otherwise the human race would cease to exist. But we mean by a living wage something more than a bare pittance sufficient to maintain a man's physical health at a proper standard. This is the criterion for an ox or an ass, not a human being. We mean a wage suitable to the development of the physical, moral, and intellectual attributes of mankind. This is what Mr. Clynes, one of the clearest thinkers in the labour world to-day, means by a living wage. He defines it as one which should ensure to the human being a condition of life 'equal to the expectations and tastes of a civilised population of this age.' This is an ideal which we should all, I think, readily accept. But I must emphasise the point that it is an *ideal*, and that therefore it may not be capable of realisation in all times and in all places. Wages, as I have pointed out above, depend on the accumulated wealth of a community, which is obviously greater in times of progress and development than during a period of stagnation or retrogression. Clearly, therefore, wages must vary from time to time, and it is idle to pretend that any country can guarantee permanently a wage equal to the expectations and tastes of a civilised population. We are now living in a period of industrial stagnation, following upon one of intense activity. It is inevitable, therefore, that wages should fall. It is inevitable, too, that the wages in one industrial country should approximate to those of others where competition for foreign markets is concerned. Unless we have greater natural advantages than our foreign rivals, or are more industrious, or have superior mechanical contrivances, we cannot pay higher wages here than there, for if we do they will underquote us and take away our foreign trade, which is essential to our existence. And this is just what is happening to-day. It is clear, therefore, that a lowered standard of civilisation in one country will react disastrously on others, and that if the more fortunate are not willing to lend a helping hand to their poorer neighbours they will themselves be dragged down to the same level. Instead of trying to keep Germany under, we ought,

therefore, to try to put her on her feet, not merely as a moral duty, but on the lowest grounds of self-interest.

I have dwelt at some length on the point that a civilised wage such as we all desire may be unattainable, because it is of critical importance to-day, and because, obvious though it may appear, it is widely ignored. We are continually being told that the standard wage should be the 1914 wage, plus a percentage equivalent to the increase in the cost of living since that date. And yet we are obviously poorer than we were in 1914, and it is equally obvious that our foreign trade is slipping from our grasp, owing to the competition of Germany and America. From a practical point of view what is necessary is not to work out a standard wage which we should like to pay if we could, but to determine what wages we can afford to pay in each industry without losing our foreign markets. This can only be settled by a frank discussion between employers and employed, and it is essential that employers should disclose all the facts. This would reveal that in many industries prices have fallen faster than costs, and that work is being taken at a loss. This is, I believe, right as a temporary measure, because it is not reasonable that all the sacrifices should be borne by the workers. But it can be only temporary, otherwise fresh capital will not be forthcoming, and our industries will perish for the want of it.

It is clear, therefore, that in accepting the principle of a civilised wage we must have due regard to the progress, maintenance, and well-being of the industry under consideration.

But it may be that, whilst the great majority of trades and industries in the country can afford to pay what may fairly be regarded as a civilised wage, some few industries may be unable to do so. One way of meeting the difficulty is by the imposition of a special tariff on imported goods, on the lines of the Safeguarding of British Industries Bill. I must not be led too far astray into the byways of controversy, but I confess that I think this is a thoroughly bad solution. I do not object to the protection of infant industries, but if a full-grown industry cannot walk without crutches we are better without it, even if its absence may embarrass us in a world war once every hundred years. As a matter of fact, however, sweated wages are usually the result of inefficiency—absence of labour-saving devices and bad organisation. So that often the real remedy for a trade in which wages are depressed is an expert inquiry into its methods of working, and State-aided scientific research, which has an important field ahead of it.

If it is accepted that the basic wage of a worker must be a living wage and that this term should be interpreted as liberally as possible consistently with the progress, maintenance, and well-being of the industries of the country, a further question arises. What do we mean by a worker? Do we mean a single man, a childless married man, or a man with a family? Obviously, I think, the cost of living for a married man with a family is greater than for a single man, although I have heard the opposite argued, very unconvincingly. Is a living wage to cover the expenses of a married man with an average family of, say, three children? Or should it merely cover the man, some other means being found to provide for the wife and family? The case for a

single-man wage plus family allowances has recently been put forward with great ability by Miss E. F. Rathbone. She points out that 27 per cent. of the men workers over twenty in England are bachelors or widowers without dependent children; 24.7 per cent. are married couples without children or with no dependent child below fourteen; 16.6 per cent. have one dependent child; 13 per cent. have two dependent children; 8.8 per cent. have three dependent children; and 9.9 per cent. have more than three dependent children. Hence a living wage based on the five-member family is adapted to the needs only of one of the smallest actual groupings. She argues, therefore, that the childless man is getting too high a wage in relation to the man with a family, and that the distribution of the wage fund is uneconomical. Her suggestion is that the wives and children should be provided for out of a separate fund maintained by contributions from employers calculated according to the number of their male employees, whether married or single. Thus the employer will have no inducement to prefer single to married men, whilst every wage-earner and his family will be assured of an income at least adequate to the needs of healthy physical subsistence, and at the same time the wages bill of the country will be substantially reduced, thus relieving industry of a burden that is threatening to strangle it. In support of her proposals, she points out that, so far as the children are concerned, this plan has already been embodied in the New South Wales 'Maintenance of Children Bill,' and that Mr. Hughes has foreshadowed the intention of the Federal Government of Australia to introduce a similar Bill into the Federal Parliament. It may be added that a scheme similar to that outlined by Miss Rathbone has actually been adopted by the textile industries of the Roubaix-Tourcoing district of France.

Even if it be assumed that Miss Rathbone's scheme would have the results that are claimed for it, I am strongly of opinion that the remedy would prove far worse than the disease. In the first place, it will, I am convinced, prove impossible to confine this scheme to the wage-earners; it must be extended to the salaried classes, and, in fact, to the whole community. It will thus inevitably fall to be administered by the State, and I confess that I can imagine no more detestable form of State Socialism. For it will involve a State interference in the home life which will make the war-time activities of the Government fade into insignificance. In the second place, however much we may attempt to disguise the fact, the effect of a family fund must be to subsidise families at the expense of the childless. I can see no justification for the argument that a wife and family are a burden which no man can reasonably be expected to bear, and from which, therefore, he must be relieved by the State or his more fortunate celibate fellow-citizens. Nor is his marriage necessarily a benefit to the community to be gratefully acknowledged by a dole. In fact, I can imagine the Dean of St. Paul's, for example, arguing that a premium should be paid to those who do not increase the population of the country. All virile and healthy nations have recognised that the husband is responsible for the maintenance of his family, that he must be regarded as the bread-winner, and that only thus can the family be so closely knit together that it is

in the true sense of the word a unit. In my opinion the State has already gone too far in the direction of taking over the duties of parents, and I regard as deplorable the present tendency to throw more and more of the burden in respect of housing, medical attendance, dentistry, food, clothing, and education on the State, thus relieving parents of what should in large measure be their own responsibility. This policy breeds up a race of slaves—not free men.

I have dealt so far with two principles by which wages are determined—the law of supply and demand, and the principle of a living wage. I will now pass on to a third—the principle that wages should be proportioned to the service rendered. In some respects this result is achieved through the operation of the law of supply and demand, and in the last resort the price that one man is prepared to take and another to give for labour is the only practical criterion of its value. But the value of the service rendered is not merely the result of a haggling match. It is clearly just, for example, that a good worker should receive higher wages than a bad one, that the man who produces much should be paid more than the man who produces little. By far the best way of securing this end is through the establishment wherever possible of a piece-work or premium bonus system. Under such a system not only does the payment received bear a direct relation to the results attained, it also acts as an incentive to greater effort. It might naturally be expected therefore that the Trade Union Movement would encourage a system that has such obvious advantages. Unfortunately, however, some of the leading Trade Unions are opposed to payment by results, and, if they cannot suppress it altogether, are successful in preventing its extension. The explanation of their attitude, of course, is a fear, often well justified by past experience, that payment by results will lead to speeding-up followed by an arbitrary reduction in the rates. It will lead me too far afield if I attempt a detailed discussion of this question, and I will merely say the objections, though serious, are by no means insuperable. Some firms, for example, have agreed that if an alteration is made in their piece-work list the saving shall always be used to increase the wages of some other section of their workers. In other cases a piece-work list is mutually agreed by representatives of employers and workers, and can only be altered after negotiation. I should like to emphasise the importance to our national industries of encouraging payment by results, because it is one of the surest ways of increasing efficiency. If the principle were accepted by both labour and capital, as it should be, a frank discussion would disclose the means of overcoming the abuses that experience has proved to exist.

But there will always be many classes of work where payment by results is not practicable, and where a time rate must be adhered to. Should some differentiation be attempted in respect of time rates? The period of maximum efficiency lies between the ages of thirty and fifty, and it has been suggested that wages should be based on a sliding scale according to age, reaching a maximum at the age of thirty and tapering off after the age of fifty. Any cut-and-dried rule of this description would be most objectionable and work with great harshness. There are many men over fifty who are far more efficient than younger men, and

it would be unjust to place them on a lower scale of wages. Moreover, the result of such a sliding scale would be to give an undue preference to older men, when employment is scarce, at the expense of younger men with growing families. At the same time, something should be done to meet the case of the old, the infirm, and the maimed, although the matter cannot be left to the sole discretion of employers without serious risks. In Australia the arbitration laws empower arbitrators to license old, slow or infirm workers at lower rates, and I think the same principles should be adopted here. In particular, wounded soldiers in receipt of a pension should be licensed to accept lower rates where their working efficiency has been impaired, since it is an injustice that men who have been wounded in the defence of their country should be handicapped in getting work.

The principle of equal pay for the same work leads on to the consideration of women's wages. We are all familiar with the battle-cry of 'equal pay for equal work' which has a convincing ring about it. But when one considers it more closely one realises that it is extremely difficult to define what is meant by equal work. How are you to compare the work of a hospital nurse and a stockbroker, or the services of a charwoman and a sailor? A comparison between the work of men and women is possible where both are doing identically similar jobs, though even here there are many different factors which make it difficult. But in practice the tendency is for men and women to do different types of work. During the war women to a large extent replaced men in the factories, but their introduction nearly always led to a reclassification of the work. Except in the earliest days of the war, men and women did not work indiscriminately at the same jobs; certain classes of work were allocated to them, and, as time went on, they were usually collected into separate shops. Since the war men have largely reverted to their old jobs, replacing women, and the tendency to differentiate between the spheres of men and women grows more and more marked. It is the exception that they do the same work as men, and a comparison between the value of their work and man's in his different sphere serves no useful purpose. Their wages must largely be governed by the law of supply and demand, and since the openings for women are relatively fewer than for men; since for a variety of reasons their cost of living is lower; and since their average term of service is shorter and therefore their experience is less than that of men, because they usually cease work on marriage, it is inevitable that, on the whole, their wages should be lower than those paid to men.

The three main principles governing wages, then, are the law of supply and demand implying freedom of contract or a willing buyer and a willing seller, the cost of living, and the principle that wages shall be proportioned to the service rendered. There are certain other considerations referred to by Adam Smith as leading to inequality of wages which may be briefly mentioned. They are (1) The agreeableness or disagreeableness of the work to be done. (2) The expense of learning a trade or profession. (3) Constancy of employment. (4) Responsibility. (5) The risk of failure.

It has been suggested that another factor in determining wages in

any given industry should be the financial prosperity of that industry, that wages should bear some definite relation to profits and presumably to losses, although this fact is seldom emphasised. Profit-sharing may, or may not, form a valuable adjunct to the wages system, but no form of co-partnership or of the co-operative movement can ever replace the wage system, for the simple reason that you cannot keep body and soul together on a minus quantity of food; there must always be some guaranteed minimum. The essence of the wage system is that the employee is assured of his wage whether the business makes a profit or a loss, and the fundamental wage on which those of all higher grades are based—namely, the wage of the unskilled worker—must be determined by the cost of living, not solely by considerations of profit and loss. I can see no other practicable basis for a wage system, and even under Guild Socialism or State Socialism this principle must be operative—however much the pill may be gilded by high-sounding phrases.

Wages, of course, do tend to rise in any industry when trade conditions improve, and in that sense profits are shared, but the exclusive enjoyment of the improvement does not remain for long. If, for example, the tinplate industry is prosperous, the workers in that trade are the first to feel the benefits of improved conditions. But soon the miner who supplies the coal on which the industry depends, the railway worker who transports it, the butcher and the baker who feed the tinplate worker, and so on, will also require a share. Moreover, the coal-owner and the railway company will expect consideration, so that in the end the prosperity of one industry tends to become generally diffused. And this tendency is natural, partly because of the dependence of one trade on another, and partly because the wages of one trade or employment tend to bear a definite relation to those of other trades. Hence there are strong forces always at work in the direction of equalisation, both as regards wages and profits. Again, labour, in this country, is organised on a craft, not an industrial, basis. There are fitters, turners, carpenters, joiners, boilermakers, employed in a number of different industries, and their wages are those of their craft; they are not fixed in relation to the industry for which they happen to be working. It is one of the cardinal principles of craft unionism that there should be a uniform wage for all able-bodied members of the same craft. A railway porter on the London and North Western Railway, for example, claims the same wage as his brother on the Great Central, and the latter would be outraged at the suggestion that he should accept a lower wage than the former merely because the London and North Western Railway happens to be more prosperous than the Great Central.

I think that the importance which the workers themselves attach to the maintenance of a definite relation between the wages of different classes of employment has been underrated by those who advocate profit-sharing as a panacea for all our industrial troubles. Mr. Cramp put the case very well last year when presenting the demand of the railway men for increased wages before the National Wages Board. 'So far as the workers are concerned,' he said, 'their status is deter-

mined to a greater degree than that of any other section of society by the amount of wages they receive. In other walks of life a man's titles or his learning or his particular standing frequently are not related to the amount of income he receives, but with the workers a man's standing is almost entirely related to his income. Men, women, and their children are judged, and their social conditions are determined, by the amount of wages or salary which they are able to earn and the consequent standard of life that they are able to maintain.' He proceeded to make a comparison between the wages of railway men and those of other callings—in particular dockers, miners, policemen, and municipal workers—with the object of showing that the pay of railway men was low in comparison with that of other walks of life.

Th fact that any particular industry is making large profits is not—*per se*—a ground for increasing the remuneration of the workers any more than the fact of a Budget surplus is a justification for increasing the salaries of Civil Servants all round. We feel that the general taxpayer is entitled to the saving, and it may be that the general public is entitled to participate in the prosperity of an industry either through a reduction in prices or through the taxation of profits.

Another objection to profit-sharing deserves a brief mention. It is that if it is to succeed the capital employed must be high in relation to the wages paid, otherwise the profits to be shared will be insignificant. Suppose, for example, that the capital employed in a business is 1,000,000*l*., and the annual wages are 3,000,000*l*., as might well happen in a shipyard; suppose, again, we assume the profit earned to be 10 per cent., which would be a very high average in the shipbuilding trade—before the War it did not, I believe, exceed 3 per cent. for the industry. If half the profits went to capital and the other half were shared between labour and capital—a very common form of profit-sharing—labour would receive 25,000*l*., or an addition of only twopence in the £ on its wages. Bearing in mind the increases that have actually taken place in recent years, it will be recognised that such an addition would be regarded as insignificant. The fact is that in any country where labour is well organised wages absorb as much as can be allotted to labour consistently with a reasonable return to capital. And if a reasonable return is not offered to capital no capital will be forthcoming.

It is extremely doubtful if labour would tolerate a different remuneration between the various firms within an industry owing to the importance attached to the maintenance of a definite relation between the wages of different groups of men. But if they were prepared to accept a differentiation other forces would counteract it. A successful firm making large profits would be able to offer higher remuneration than an unsuccessful one, and thus attract the best men. Theoretically this may seem right and proper, but in practice the unsuccessful firm would find itself obliged to guarantee a bonus to its workers equivalent to the share of the profits accruing to the workers in the more prosperous ventures. Otherwise it would find that it could only attract the least efficient workers at a time when efficiency was most needed to save it.

The objection that under a profit-sharing system there might be glaring inequalities as between one business and another has been anticipated under the so-called profit-sharing scheme recently adopted in the coal-mining industry. It was realised that any scheme would fail if a collier working on a rich mine were to receive far more than his fellow-worker on a poor mine immediately adjacent, although the type of work done by each and the hours of labour might be exactly the same. Hence the country has been divided into districts, and within each district there is no variation in the scale of pay. A standard wage is fixed in each district, being in effect the July 1914 rates plus, in the case of piece-workers, the percentage additions which were made consequent upon the reduction of hours from eight to seven; and there is a guaranteed minimum for a certain period of the standard wages plus 20 per cent. A standard profit is fixed equivalent to 17 per cent. of the cost of the standard wages, and any surplus after paying standard wages, costs of production, and standard profits is to be shared between labour and capital, 83 per cent. being applied in each district to the payment of wages above the standard wages, and 17 per cent. being allocated to the owners as profit. This is not a profit-sharing scheme in the proper sense of the term because, while the district as a whole may make a profit, and therefore wages may increase in the proportions specified, certain individual firms may make a loss and yet be obliged to pay the increased wages. It is possible that if such a scheme is to prove workable, the mines in each district will be obliged to amalgamate, for under present arrangements the poor mines will in effect be penalised by the profits of the rich ones, whilst the latter will benefit owing to the reduction in average profits due to the losses on the former. Hence the distinction between the poor and rich mines will merely be accentuated. It is more probable, however, that there will be no average profits during the next few years, since substantial reductions in the price of coal are essential and inevitable, and that the scheme will prove to be still-born. Should it, however, turn out to be a vigorous infant and lead on, as I have suggested, to district amalgamations, a further consequence will be that the miners will make a strong demand for a voice in the control of the industry. This is a natural consequence of effective profit-sharing, and one which I believe to be unworkable. It would, however, carry me beyond the scope of this paper to discuss the question, and I will merely say that I believe the ultimate control in any business must always rest with those who bear the financial responsibility.

Whilst I do not believe that profit-sharing will ever solve the problem of the fair distribution of the proceeds of industry between labour and capital, it may prove of advantage in particular cases, and it is to be hoped that experiments will continue to be made in this direction. In fact, there is much to be gained by experiments in as many directions as possible. Co-partnership, the co-operative movement, which is a form of profit-sharing among consumers, building and other guilds, State and municipal management, individual enterprise—all have a part to play in satisfying the various demands of human nature, and there is room for all. For elasticity is an essential

feature of successful industrial development, and the individual liberty which this implies can only be found in countries where the law is respected and there is a strong Government. For, as has been truly said, 'Where order reigns her sister liberty cannot be far.' The outstanding feature in the history of our own Empire, as of every successful commercial community, and the lesson of Europe to-day, is that industrial prosperity depends upon stable political conditions combined with individual liberty. The absence of either always has resulted and always will result in industrial stagnation and disaster.

The conclusions, then, that I would put before you are these: There is no simple and straightforward system applicable to the division of the proceeds of industry between labour and capital. Both are essential to industry, and therefore to each other; hence the deeper interests of both lie in co-operation, and the task before the leaders of labour and of capital consists in promoting the interests of both, not in selfishly pursuing the advantage of the one at the expense of the other. Both must recognise the need of contenting the other, for if capital is not satisfied its springs will dry up and the industrial body will wither away, whilst if labour is discontented and the members of the industrial body war against each other the end is death. The real solution of the problem is a moral one, and can be achieved only if justice and virtue govern the lives of the members of the community, for all human organisations must reflect the character of those who work them. Arbitration offers no immediate solution of the difficulty, for to be effective it must be voluntarily accepted by the majority on both sides, and the principles by which arbitrators are to be guided must first be clearly expressed and accepted. But it is the goal at which civilisation must aim, and as a step in this direction public inquiries into all disputes between labour and capital should be encouraged after all attempts at mutual agreement have failed.

A clearer understanding of economic truths in the industrial world is essential if disputes are to be avoided. It must be recognised that the wealth available for wages depends on the total production of the country, and that whilst, if production increases, wages will go up, if it falls wages must come down. It must be recognised, too, that where foreign competition is concerned the wages paid in one industrial country must have an important bearing on those paid in others.

So long as the present industrial system continues—and no alternative system that is practicable at any rate within a measurable distance of time has ever been suggested—the wages system must prevail. Profit-sharing is no substitute for it, since, amongst other reasons which I have referred to, the amount necessary to provide a living wage will and should absorb practically the whole of the share available for labour, leaving only a reasonable return to capital sufficient to encourage its production.

The fundamental wage, or the wage of unskilled labour, should be a living wage—that is, a wage suitable to the development of the physical, moral, and intellectual attributes of the citizens of a free country. But it must be recognised that the degree to which this ideal can be attained must depend on the skill and endeavour of the people,

and due regard must be had to the progress, maintenance, and well-being of the industries of the country. It is idle to hope that the living wage can be based permanently on any given standard of civilisation; it is bound to fluctuate at different periods, and will depend largely on whether the industries of a country are progressive, stagnant, or retrogressive. Wages above the minimum or living wage are determined mainly by the law of supply and demand, but certain other factors enter into their determination; notably the principle that wages should be proportioned to the value of the service rendered, implying payment by results. There never was a time when it was more important that all should grasp, not merely what is possible, but what is reasonable as regards wages. For the artificial prosperity and trade activity that followed upon the War are at an end, and the reaction has begun. How long it will last no one can tell, but it is reasonably certain that we must expect a period of depression and falling wages and profits. It is essential that the wage-earners should recognise that reductions are inevitable, and not the fault of the capitalists; they should be satisfied that all reductions proposed are reasonable. The capitalist, on his side, must be prepared to accept his full share of sacrifice, and be ready, if need be, as a temporary measure, not merely to receive no profits, but to face a loss, in order that our difficulties may be tided over until our trade recovers and prosperity returns.

NOTES ON WATER POWER DEVELOPMENT.

ADDRESS BY
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PRESIDENT OF THE SECTION.

THE extent to which the water powers of the world have been investigated and developed during the past decade forms one of the striking engineering features of the period. Although falling or flowing water formed the earliest of the natural sources of energy to be utilised for industrial purposes, it is of interest to note that two-thirds of the water power at present in use has been developed within the last ten years.

The reasons for the revival of interest in this question are partly technical and partly economic.

The technical development of electric generation and transmission has made it economically possible to utilise powers remote from any industrial centre, while a rapid increase in the demand for energy for general industrial purposes and for the many electro-chemical, electro-physical, and electro-metallurgical processes which are now in general use, and whose field is rapidly growing, has provided a ready outlet for all such energy as could be cheaply developed.

The urgent demand for energy to supply the abnormal requirements of the war period, combined with the world shortage of fuel, was responsible for an unprecedented rate of development in most countries with available water power resources, and especially in those countries normally dependent on imported fuel.

Thus in France some 850,000 water horse-power has been put into commission since 1915, and the country now has 1,600,000 horse-power under control as compared with 750,000 before the war. In Switzerland some 600,000 horse-power has been developed since 1914, or is in course of construction, as compared with 880,000 horse-power before the war. In Spain, where the pre-war output was 150,000 horse-power, the present output is 620,000 horse-power, and about 260,000 horse-power is now in course of development, while the Spanish Ministerio de Fomento is considering the development of some 2,000,000 horse-power to be delivered into a network of transmission lines covering the industrial parts of the country.

In Italy, schemes totalling about 300,000 horse-power are under way, and it is estimated that the total output will shortly amount to 2,000,000 horse-power. The Government Hydrographical Department is now engaged in gauging and surveying the profiles of the principal

rivers, and statistics of available reservoir sites, of lakes suitable for storage and of available horse-power are being compiled.

Japan, which only very recently began to investigate her water powers, has, since 1916, developed over 1,000,000 horse-power, or almost twenty per cent. of her available resources.

In Canada and the United States many large schemes have recently been brought into service, and some extremely large installations are now in course of construction or are projected. Thus the Queenston-Chippewa project on the Canadian side of the Niagara River is intended to develop some 500,000 horse-power, while a projected development of the St. Lawrence River will be capable of yielding 1,700,000 horse-power. In Canada the total development (2.3 million horse-power) in 1918 was almost three times as great as in 1910. In the United States of America the development has increased from something under two million horse-power in 1901, to 5.3 millions in 1908, and to nearly 10.0 millions in 1920.

Rapid as has been the development of water power in the United States in the past, it has been retarded by the fact that the privilege of using the national forests or other public lands for water power development has only been granted by the issuing of permits which were not available for any definite period and which were revocable at the will of the Granting Authority. In the case of development on navigable streams, whether on public or private land, each scheme has required a special Act of Congress, and these Acts could be revoked by Congress at any time. Owing to the uncertainty of tenure there has naturally been some reluctance to invest capital in such undertakings.

By the recent Federal Water Power Act, signed in June 1920, licences for such developments may now be issued under the jurisdiction of a new body, known as the Federal Power Commission, for a period not exceeding fifty years, at the end of which the licence may be renewed, or the Government may take over the enterprise upon compensation of the licensee. In the issuing of licences, preference is to be given to State and municipal applications. The effect of this Act may be inferred from the fact that, within a month of its being signed, applications for licences to develop over 500,000 horse-power had been filed. The duty of collecting, recording, and publishing data regarding the utilisation of water resources, the water-power industry and its relation to other industries, and regarding the capacity, development costs, and relationship to possible markets, of power sites, has also been assigned to this Federal Power Commission.

World's Available Water Power.—During the past few years much attention has been paid to statistics of available and developed water powers. In the case of developed powers, these are usually stated in terms of the capacity of the installed machinery. This machinery is in general only used to its full capacity over a portion of each day, although in many such cases water is available for providing continuous power if desired.

Estimates of potential power are always to be accepted with considerable reserve. In order to make a reasonably accurate estimate,

the run off from the catchment area and the variation in this run off from month to month and from year to year, must be known, and it is only in comparatively rare cases that this information is as yet available. Moreover, there is as yet no standard basis on which potential power is computed.

The power available from a given stream during the wet season is many times as great as during the dry season unless sufficient storage is available to equalise the flow throughout the year, and the cost of such storage would in general be prohibitive, even if it were physically possible to provide it.

The United States Geological Survey takes the maximum useful flow of a stream as being that which may be guaranteed during six months in each year. The minimum flow is taken as the average which can be guaranteed over the two driest consecutive seven-day periods in each year, along with the additional flow which may be obtained during this period by developing any available storage capacity in the upper waters of the stream. Estimates of potential power based on storage capacity are, however, subject to a wide margin of error owing to the limited data available, and in the following table the potential water power is estimated on the basis of the maximum flow as just defined, and in terms of continuous twenty-four-hour power.

(*Millions of Horse Power.*)

—		Available	Developed
Great Britain		0·9	0·2
Canada		23·0	3·28[1]
Remainder of British Empire including	Australia Africa (East) „ (South) „ (West) British Guiana India and Ceylon New Zealand Papua	30·0 to 50·0	0·7
Austria		6·5	0·57
Brazil		26·0	0·32
Dutch East Indies		5·5	—
France		5·6	1·6
Germany		1·5	0·75
Iceland		4·0	1·0[2]
Italy		4·0	1·25
Japan		8·0	1·5
Norway		7·5	1·25
Russia		20·0	1·0
Spain		5·0	0·88
Sweden		6·2	1·2
Switzerland		4·0	1·4
United States of America		28·0	9·8

Adopting these figures it appears that the available horse-power of the world is of the order of two hundred millions, of which

[1] Including projected extensions to plants now in operation.
[2] Projected but not yet constructed.

approximately twenty-five millions is at present developed or is in course of development.

Power Available in Great Britain and in the British Empire.—With the noteworthy exceptions of Canada and New Zealand, practically nothing had been done, prior to 1915, by any part of the British Empire, to develop or even systematically to investigate the possibilities of developing its water powers. It is true that a number of large installations had been constructed in India and Tasmania, but their aggregate output was relatively inconsiderable.

Since then, however, there has been a general tendency to initiate such investigations, and at the present time these are being carried out with varying degrees of thoroughness in India, Ceylon, Australia, South and East Africa, and British Guiana. While it is known that there is ample water power in Newfoundland, Nigeria, Rhodesia, Papua, and the Gold Coast, no very definite information is available, nor are any steps apparently being taken to obtain data in these countries.

The Water-power Committee of the Conjoint Board of Scientific Societies, which has been studying the state of investigation and development throughout the Empire since 1917, has, however, come to the conclusion that its total available water power resources are at least equivalent to between 50 and 70 million horse-power.

Of the developed power in the Empire about 80 per cent. is in Canada. Throughout the remainder of its territories only about 700,000 horse-power is as yet developed, or only a little over 1 per cent. of the power available, a figure which compares with about 24 per cent. for the whole of Europe, and 21 per cent. for North America, including Canada and the U.S.A. These figures sufficiently indicate the relatively large scope for future development.

Power Available in Great Britain.—With a view of ascertaining the resources of our own islands, a Board of Trade Water Power Resources Committee was appointed in 1918. This Committee, which has just presented its final report, has carried out preliminary surveys of as many of the more promising sites as its limited funds allowed, and has obtained data from the Board of Agriculture for Scotland, the Ordnance Survey Department, the Ministry of Munitions, and from civil engineers in private practice, regarding a large number of other sites.

As might be anticipated, Scotland, with its comparatively high rainfall, mountainous area, and natural lochs, possesses relatively greater possibilities than the remainder of the United Kingdom, and investigation has shown that it offers a number of comparatively large schemes. Nine of the more immediately promising of those examined by the Committee have an average output ranging from 7,000 to 40,000 continuous 24-hour horse-power, and an aggregate capacity of 183,000 horse-power, while in every case the estimated cost of construction is such that power could be developed at a cost appreciably less than from a coal-fired station built and operated under present-day conditions. The aggregate output of the Scottish schemes brought before the notice of the Committee, some of which, however, are not commercially feasible at the moment, is roughly 270,000 continuous horse-power.

In addition to these there are a very large number of other small schemes which have not yet been investigated,[1] and it is probably well within the mark to say that there are water-power sites in the country capable of developing the equivalent of 400,000 continuous horse-power, or 1,500,000 horse-power over a normal working week, at least as cheaply as from a coal-fired installation.

A number of attractive schemes are also available in North Wales, though these are in general more expensive than those in Scotland.

Owing to the general flatness of the gradients, there are, except possibly around Dartmoor, no schemes of any large individual magnitude in England, but there are a large number of powers ranging from 100 to 1,000 horse-power which might be developed from river flow uncontrolled by storage.

Investigations on a few typical watersheds throughout England and Wales appear to show that the possible output averages approximately eight continuous horse-power per square mile of catchment area, which would be equivalent to an aggregate of about 450,000 horse-power. Although much of this potential output is not commercially feasible, it would give the equivalent of 500,000 horse-power over a normal working week if only 30 per cent. of it were fully utilised.

In the report recently issued by the Irish Sub-Committee of the Board of Trade Water Power Committee, it is estimated that approximately 500,000 continuous 24-hour horse-power is commercially available in Ireland, and that if utilised over a 48-hour working week, its capacity would be at least seven times as great as that of the engine power at present installed in the country for industrial purposes.

It appears then that, although the water power possibilities of the United Kingdom are small in comparison with those of some more favoured countries, they are by no means so negligible as is commonly supposed, even in comparison with the present industrial steam power resources of the country.

The capacity of the fuel power plants installed for industrial and public utility services in the United Kingdom in 1907 was approximately 9.8 million horse-power. Allowing for an increase of 15 per cent. since then, and an average load factor of 35 per cent., this is equivalent to 32,000 million horse-power hours per annum, or to a continuous 24-hour output of only 3.7 million horse-power.

According to Sir Dugald Clerk, the average consumption of coal per horse-power hour in this country is about 3.9 lb., which, on the above basis, would involve a total annual consumption of 55 million tons for industrial purposes, not including railways or steamships. This figure is in substantial agreement with the estimate of 60 million tons made for factory consumption in 1913 by the Coal Conservation Committee of the Ministry of Reconstruction, since this latter figure also includes coal used for heating and other manufacturing processes in factories.

[1] In a paper read before the Royal Society of Arts on January 25, 1918, Mr. A. Newlands, M.I.C.E., gave a list of 122 potential Scottish schemes, the capacity of which he estimated, on a very conservative basis, at 375,000 horse-power.

Adopting this figure of 32,000 million horse-power hours as the annual demand for power for industrial purposes, it appears that the inland water power resources of the United Kingdom are capable of supplying about 27 per cent. of this, a proportion which, in such an industrial country as our own, is somewhat surprisingly large.

Many of the small powers would be well adapted for linking up, as automatic or semi-automatic stations, into a general network of electricity supply, or for augmenting the output of municipal supply works, as has been done so successfully, for example, at Chester, Worcester, and Salisbury.

The development of the many small schemes available in the Scottish Highlands would probably have a great effect on the social life of the community. It would go far towards reviving and extending those small local industries which should form an essential feature of the ideal rural township. Commercially such undertakings may appear to be of small importance, but as a factor in promoting the welfare of the State, economical and political, their influence can hardly be over-estimated.

Some of the larger schemes in Scotland would lend themselves admirably to transmission to its industrial districts, while others, in close vicinity to the sea-board, would appear to be well adapted for supplying chemical, or electro-physical, or metallurgical processes.

There is a probability that at least two of these schemes will be developed in the near future. One of these—the Lochaber scheme—is capable of developing some 72,000 continuous horse-power, which is to be utilised largely in the manufacture of aluminium. It is interesting to note that when this scheme is completed the British Aluminium Corporation will have, with their station at Kinlochleven, an average continuous output of over 100,000 horse-power, and a maximum capacity of almost 140,000 horse-power.

The second—the scheme of the Grampian Power Company—is intended ultimately to develop upwards of 40,000 continuous horse-power, which it is proposed to use largely for general industrial purposes.

Should this latter scheme be carried to a successful conclusion it is likely to give an impetus to large-scale water power development in Scotland. Its successful operation would certainly lead to the development of others of the same type, which would help to provide a much needed home training ground for British hydro-electric engineers.

While this is admittedly an inopportune moment to suggest anything in the nature of State co-operation in such developments, it may be pointed out that many of the Scottish powers in particular occur in sparsely populated districts, and that, although they would ultimately become remunerative, the difficulty of raising the capital necessary for their development is great. In view of their direct and indirect advantage to the community it would appear not unreasonable to advocate that financial assistance should be granted by the State in the earlier stages of such developments. If such assistance, say in the form of a loan maturing after a period of ten or fifteen years, could be granted, it would certainly give an immediate impetus to the development of water power in this country.

Conservation.—The importance of water power development from the point of view of conservation of natural resources requires no emphasis. When the value of coal purely as a chemical asset, or as a factor in the manufacture of such materials as iron and steel, cement, &c., is considered, its use as a fuel for power purposes, when any other equally cheap source of energy is available, would appear, indeed, to be unjustifiable.

The consumption of coal in the best modern steam plant of large size, giving continuous output, would be about nine tons per horse-power year, and on this basis the world's available water power if utilised would be equivalent to some 1,800,000,000 tons of coal per annum. The world's output of coal in 1913 was approximately 1,200,000,000 tons, of which about 500,000,000 tons were used for industrial power purposes, so that on this basis 55,000,000 continuous water horse-power would be equivalent to the world's industrial energy at that date.

Not only does the use of water power lead to a direct conservation of fuel resources but it also serves to a notable degree to conserve man power. To take an extreme example, each of the 40,000 horse-power units now being installed at Niagara Falls will require for operation two men per shift. It is estimated that to produce the same power from a series of small factory steam plants, over eight hundred men would be required to mine, hoist, screen, load, transport by rail, unload, and fire under boilers the coal required, while, if account be taken of the additional labour involved in horse transport, wear and tear of roads and of railroad tracks and rolling stock, the number would be considerably increased.

Uses of Hydro-Electric Energy.—While a large proportion of the energy developed from water power is utilised for industrial purposes and for lighting, power, and traction, an increasing proportion is being used for electro-chemical and electro-metallurgical processes. It is probable indeed that we are only on the threshold of developments in electro-chemistry, and that the future demand for energy for such processes will be extremely large.

In Norway the electro-chemical industry absorbed 770,000 horse-power in 1918, or approximately 75 per cent. of the total output, as compared with 1,500 horse-power in 1910. Of this some 400,000 horse-power was utilised in nitrogen fixation alone.

The production of electric steel in the U.S.A. increased from 13,700 tons in 1909 to 24,000 tons in 1914, and to 511,000 tons in 1918, this latter quantity absorbing 300 million kw. hours, equivalent to almost 400,000 continuous horse-power.

In Canada, in 1918, the pulp and paper industry absorbed 450,000 horse-power, or 20 per cent. of the total, while the output of central electric stations amounted to 70 per cent. of the total.

The electrification, on a large scale, of trunk line railways is also a probability in the not distant future. In the U.S.A. 650 miles of the main line of the Chicago, Milwaukee, and St. Paul Railway, comprising 850 miles of track, have been electrified, the power for operation being obtained from hydro-electric stations. In France much of the track of the Compagnie du Midi in the region of the Pyrenees has

been electrified with the aid of water power; much of the Swiss railway system has been electrified; and the electrification of many other trunk lines on the European continent is at present under consideration.

Quite apart from the probable huge demand in the distant future for energy for the manufacture of artificial fertilisers by some system of nitrogen fixation, agriculture would appear to offer a promising field for the use of hydro-electric power.

Much energy is now being utilised in the U.S.A. for purely agricultural purposes. In California, for example, there is in effect one vast system of electrical supply extending over a distance of 800 miles with 7,200 miles of high-tension transmission lines. This is fed from seventy-five hydro-electric stations, inter-connected with forty-seven steam plants, to give a total output of 785,000 horse-power. A further group of thirteen hydro-electric schemes now under construction will add another 520,000 horse-power. A large proportion of this power is used in agriculture, and a census in 1915 showed that electric motors equivalent to over 190,000 horse-power were installed on Californian farms. The Californian rice industry is almost wholly dependent on irrigation made possible by electric pumping, while most of the mechanical processes involved in farming are being performed by electric power.

There can be little doubt that the economic development of many of our tropical dependencies is bound up in the development of their water power resources. Not only would this enable railroads to be operated, irrigation schemes to be developed, and mineral deposits to be mined and worked, but it would go far to solve the black labour problem, which promises to be one of some difficulty in the near future.

While those outlets for electrical energy which are now in sight promise to absorb all the energy which can be cheaply developed for many years to come, there are many other probable directions in which cheap energy would find a new and profitable outlet. Among these may be mentioned the purification of municipal water supplies; the sterilisation of sewage; the dehydration of food products; and the preservation of timber.

Scope for future Water Power Development.—The figures already quoted indicate that the scope for inland water power development throughout the world, and more particularly throughout the British Empire, is likely to be large for many years to come, and it is gratifying to know that British engineers are prepared to play a large part in such development work.

The utilisation of this water power is likely to give rise to some economic problems of interest and importance. When industrial conditions have again become stabilised, the competitive ability of the various nations will depend largely on economy in the application of energy to production and transportation, and the possession of cheap water power is likely largely to counterbalance the possession of such resources as coal and iron as a measure of the industrial capacity of a nation.

While it is probably true in industrial communities that the most attractive water power schemes have already received attention, many

of those available in countries which have hitherto been non-industrial are capable of extremely cheap development and will certainly be utilised as soon as a market for their output can be assured.

It is in such countries that the result of these developments is likely to be most marked, and will require most careful consideration. Thus the hydro-electric survey of India now being carried out by the Indian Government indicates that very large water power resources are available in the country, and that, although a few large schemes have been or are being developed, the resources of the country are practically untouched. There can be little doubt that in the course of time a large amount of cheap energy will be available in India for use in industrial processes, and as the country possesses a large and prolific population readily trained to mechanical and industrial processes, along with ample supplies of raw material for many such processes, all the conditions would appear to be favourable for its entry into the rank of manufacturing and industrial nations.

Modern Tendencies in Water Power Development.—The large amount of attention which has been concentrated on the various aspects of water power development during the past ten years has been responsible for great modifications and improvements in the design, arrangement, and construction of the plant.

Broadly speaking these have been in the direction of increasing the size, capacity, reliability, and efficiency of individual units; of improving the design of the turbine setting and of the head and tail works; of increasing the rotative speed of low head turbines; of detailed modifications in the reaction type of turbine to enable it to operate under higher heads than have hitherto been considered feasible; and of increasing the voltage utilised in transmission.

The capacity of individual units has been increased rapidly during recent years, and at the present time units having a maximum capacity of 55,000 horse-power under a head of 305 feet are being installed in the Queenston-Cheppewa project at Niagara, while units of 100,000 horse-power are projected for an extension of the same plant.

These modern high-power turbines are usually of the vertical shaft, single runner type, with the weight of the shaft, runner, and generator carried from a single thrust bearing of the Michell type. This type lends itself to a simple and efficient form of setting, while the friction losses in the turbine are extremely low. As a result of careful overall design it has been found possible to build units of this type having an efficiency of approximately 93 per cent.

One of the great drawbacks of the low head turbine in the past has been its relatively slow speed of rotation, which necessitated either a slow speed, and consequently costly generator, or expensive gearing. As a result of experiment it has, however, been possible so to modify the form of the runner as greatly to increase the speed of rotation under a given head without seriously reducing the efficiency.

Investigations in this direction are still in progress and promise to give rise to important results. At the present time, however, turbines are in existence which are capable of working efficiently at speeds at

least five times as great as would have been thought feasible ten years ago.

The non-provision of a suitable pipe line has, until recent years, tended to retard the development of plants for very high heads. Under such heads the necessary wall thickness, even with a moderate pipe diameter, becomes too great to permit of the use of riveted joints. Recent developments in electric welding and oxy-acetylene welding have, however, rendered it possible to construct suitable welded pipes, and by their aid, and by the use of solid drawn steel pipes in extreme cases, it has been found possible to harness some very high falls. The highest as yet utilised is at the Fully installation in Switzerland. Here the working head is 5,412 feet, corresponding to a working pressure of 2,360 lb. sq. in. The pipe line is 19.7 in. in diameter and $1\frac{3}{4}$ in. thick at its lower end, and each of the three Pelton wheels in the power house develops 3,000 horse-power, with an efficiency of 82 per cent.

Until comparatively recently the Pelton wheel was looked upon as the only practicable turbine for high heads, and the use of the Francis turbine was restricted to heads below about 400 feet. This was due partly to the fact that a reaction turbine of comparatively small dimensions gives a large output under a high head, and except in turbines of comparatively large power the water passages become very small, and the friction losses in consequence large.

A further and more important reason for the general choice of the Pelton wheel for high heads was the fact that in the earlier Francis turbines, when operating under heads involving high speeds of water flow, corrosion of the runner was very serious. This corrosion is now generally attributed to the liberation of air containing nascent oxygen, at points where eddy formation causes regions of low pressure. Careful design of the vanes has enabled this to be largely prevented in modern runners, and in consequence the field of useful application of the Francis turbine has been extended until at present turbines of this type are operating successfully under a head of 850 feet, and this limit will probably be exceeded in the near future.

The great increase in all constructional costs since 1914 has increased the cost of the average hydro-electric plant by something of the order of 150 per cent., and since the cost of energy produced by such a plant is mainly due to fixed charges on the capital expenditure, this cost has gone up in an even greater proportion owing to the higher interest charges now demanded.

It is true that the same increased cost applies within narrow limits to the output from every steam plant erected since the War, and the relative position of the two types of power plant with coal at about 25*s*. per ton is much the same as before the War.

The fact remains, however, that a newly constructed hydro-electric plant has often to compete in the market with a steam plant built in pre-war days whose standing charges are comparatively low, and in order to enable it to do so with success the constructional cost must often be reduced to a minimum compatible with safe and efficient operation. With this in view many modifications in design and construction have been introduced in recent plants, but there would still

appear to be ample scope for investigation into the possibility of reducing the first cost by modifying many of the details of design and methods of construction now in common use.

Among recent modifications in this direction may be mentioned:—

1. The elimination of the dam in storage schemes in which natural lochs or reservoirs are utilised, this water level being drawn down in times of drought instead of being raised in times of flood. This reduces the cost of construction appreciably in favourable circumstances, and eliminates the necessity for paying compensation for flooding of the land surrounding the reservoir.
2. The substitution, where feasible, of rockfill dams for those of masonry or monolithic concrete.
3. The introduction of outdoor installations with the minimum of power house construction.
4. The simplification of the power plant.

Some progress has already been made in these directions, and it is probable that experience based on recent installations and experimental investigations will enable considerable further progress to be made.

Research in Hydro-Electric Problems.—There are few branches of engineering in which research is more urgently required and in which it might be more directly useful.

Among the many questions still requiring investigation on the civil and mechanical side may be mentioned:—

1. *Turbines.*—Investigation of turbine corrosion as affected by the material and shape of the vanes.

Effect of erosion due to sand and silt.

Resistance to erosion offered by different materials and coatings.

Bucket design in low head high-speed turbines.

Draft tube design.

Investigation of the directions and velocities of flow in modern types of high-speed turbines.

Investigation of the degree of guidance as affected by the number of guide and runner vanes.

2. *Conduits and Pressure Tunnels.*—The design of large pipe lines under low heads with the view of reducing the weight of metal. The investigation of anti-corrosive coatings, so as to reduce the necessity for additional wall thickness to allow for corrosion.

Methods of strengthening large thin-walled pipes against bending and against external pressures.

Methods of lining open canals and of boring and lining pressure tunnels.

Effects of curvature in a canal or tunnel.

3. *Dams.*—Most efficient methods of construction and best form of section especially for rockfill and earthen dams. Best methods of producing water tightness.

4. *Run-off data.*—Since the possibility of designing an installation to develop the available power efficiently and economically depends in many cases essentially on the accuracy of the run-off data available, the possession of accurate data extending over a long series of years is of great value.

While such data may be obtained either from stream gaugings or from rainfall and evaporation records, the former method is by far the more reliable. For a reasonable degree of accuracy, however, records must be available extending over a long period of years, and at the present moment such data is available only in very few cases.

Where accurate rainfall and evaporation records are available, it is possible to obtain what is often a sufficiently close approximation to the run off, but even rainfall records are not generally at hand where they are most required, and even in a district where such records are available, they are usually confined to easily accessible points, and are seldom extended to the higher levels of a catchment area where the rainfall is greatest. Even throughout the United Kingdom our knowledge of the rainfall at elevations exceeding 500 feet is not satisfactory, and little definite is known concerning that at elevations exceeding 1,000 feet.

In this country evaporation may account for between 20 and 50 per cent. of the annual rainfall, depending on the physical characteristics of the site, its exposure, mean temperature, and the type of surface covering. In some countries evaporation may account for anything up to 100 per cent. of the rainfall. As yet, however, few records are available as to the effect of the many variables involved. An investigation devoted to the question of evaporation from water surfaces, and from surfaces covered with bare soil and with various crops, under different conditions of wind, exposure, and mean temperature, would appear to be urgently needed. If this could be combined with an extension of Vermeulle's investigation into the relationship between rainfall, evaporation, and run-off on watersheds of a few characteristic types, it would do much towards enabling an accurate estimate of the water power possibilities of any given site to be predetermined.

Even more useful results would follow the initiation of a systematic scheme of gauging applied to all streams affording potential power sites.

Among other questions which are ripe for investigation may be mentioned:—

1. The combined operation of steam and water power plants to give maximum all-round efficiency.
2. The relative advantages of high voltage D.C. and A.C. generation and transmission for short distances.
3. The operation of automatic and semi-automatic generating stations.

Tidal Power.—The question of tidal power has received much attention during the last few years. In this country it has been considered by the Water Power Resources Committee of the Board of Trade, who

have issued a special tidal power report dealing more particularly with a suggested scheme on the Severn. The outline of a specific scheme on the same estuary was published by the Ministry of Transport towards the end of 1920.

In France a special commission has been appointed by the Ministry of Public Works to consider the development of tidal power, and it has been decided to erect a 3,000 kw. experimental plant on the coast of Brittany. With the view of encouraging research the Government proposes to grant concessions, where required, for the laying down of additional installations.

The tidal rise and fall around our coasts represents an enormous amount of energy, as may be exemplified by the fact that the power obtainable from the suggested Severn installation alone, for a period of eight hours daily throughout the year, would be of the order of 450,000 horse-power.

Many suggestions for utilising the tides by the use of current motors, float-operated air compressors, and the like have been made, but the only practicable means of utilising tidal energy on any large scale would appear to involve the provision of one or more dams, impounding the water in tidal basins, and the use of the impounded water to drive turbines.

The energy thus rendered available is, however, intermittent; the average working head is low and varies daily within very wide limits, while the maximum daily output varies widely as between spring and neap tides.

If some electro-chemical or electro-physical process were available, capable of utilising an intermittent energy supply subject to variations of this kind, the value of tidal power would be greatly increased. At the moment, however, no such process is commercially available, and in order to utilise any isolated tidal scheme for normal industrial application it is necessary to provide means for converting the variable output into a continuous supply constant throughout the normal working period.

Various schemes have been suggested for obtaining a continuous output by the co-ordinated operation of two or more tidal basins separated from each other and from the sea by dams with appropriate sluice gates. This method, however, can only get over the difficulty of equalising the outputs of spring and neap tides if it be arranged that the maximum rate of output is that governed by the working head at the lowest neap tide, in which case only a small fraction of the available energy is utilised.

When a single tidal basin is used it is necessary to provide some storage system to absorb a portion of the energy during the daily and fortnightly periods of maximum output, and for this purpose the most promising method at the moment appears to involve the use of an auxiliary high level reservoir into which water is pumped when excess energy is available, to be used to drive secondary turbines as required. It is, however, possible that better methods may be devised. Storage by the use of electrically heated boilers has been suggested, and the whole field of storage is one which would probably well repay investigation.

If a sufficiently extensive electrical network were available, linking up a number of large steam and inland water power stations, a tidal power scheme might readily be connected into such a network without any storage being necessary, and this would appear to be a possibility which should not be overlooked in the case of our own country.

Investigation necessary.—A tidal power project on any large scale involves a number of special problems for the satisfactory solution of which our present data is inadequate.

Thus the effect of a barrage on the silting of a large estuary, and the exact effect on the level in the estuary and in the tidal basin at any given time can only be determined by experiment, either on a small installation, or preferably on a model of the large scheme.

Many of the hydraulic, mechanical, and electrical problems involved are comparatively new, and there is little practical experience to serve as a basis of their solution.

Among these may be mentioned:—

1. The most advantageous cycle of operations as regards working periods, mean head, and variations of head.
2. The methods of control and of sluice gate operation.
3. Effect of changes of level due to wind or waves.
4. The best form of turbine and setting and the most economical turbine capacity.
5. The possibilities of undue corrosion of turbine parts in salt water.
6. The best method of operation; constant or variable speed.
7. Whether the generators shall be geared or direct driven.
8. Whether generation shall be by direct or alternating current.

The questions of interference with navigation and with fisheries; of utilising the dam for rail or road transport across the estuary; and, above all, economic questions connected with the cost of production, and the disposal of the output of such an installation, also require the most careful consideration before a scheme of any magnitude can be embarked upon with assurance of success.

In view of the magnitude of the interests involved, and of the fact that rough preliminary estimates indicate that to-day current even for an ordinary industrial load could be supplied from such an installation at a price lower than from a steam generating station giving the same output with coal at its present price, it would appear desirable that these problems should receive adequate investigation at an early date.

Facilities for Research in Hydraulic and Cognate Problems.—In view of the considerations already outlined, and especially in view of the large part which British engineering will probably play in future water power developments, the provision on an adequate scale at some institution in this country of facilities for research on hydraulic and cognate problems connected with the development of water power is worthy of serious attention.

At present the subject is treated in the curriculum of the engineering schools of one or two of our universities, but in no case is the laboratory equipment really adequate for the purpose in question.

What is required is a research laboratory with facilities for experiments on the flow of water on a fairly large scale; for carrying out turbine tests on models of sufficient capacity to serve as a basis for design; and, if possible, working in conjunction with one or more of the hydro-electric stations already in existence, or to be installed in the country, at which certain large scale work might be carried out.

The provision of such a laboratory is at the moment under consideration in the United States, and in view of the rapidity with which the designs of hydraulic prime movers and their accessories are being improved at the moment, it would appear most desirable that the British designer, in order that the deservedly high status of his products should be maintained and enhanced, should at least have access to equal facilities, and should, if necessary, be able to submit any outstanding problems to investigation by a specially trained staff.

The extent to which our various heat engine laboratories have been able of recent years to assist in the development of the internal combustion engine, and to which our experimental tanks have assisted in the development of the shipbuilding industry, is well known to most of us, and the provision of similar facilities to assist in the development of our hydro-electric industry would probably have equally good results in this connection.

As a result of representations made by the Water Power Committee of the Board of Trade, I understand that it has now been decided to initiate a Chair of Hydro-Electric Engineering in some one university, and it is greatly to be hoped that funds may be available to enable the necessary laboratory to be designed and equipped on a scale commensurate with the importance of the work which it would be required to undertake.

THE AIMS AND BOUNDARIES OF PHYSIOLOGY.

ADDRESS BY

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PRESIDENT OF THE SECTION.

UPON the occasion of our meeting in this metropolitan city of Edinburgh, the seat of an ancient university and a great centre of medical study and practice, it has occurred to me that it may be profitable for us to consider the part which physiology should rightly take in its relation to national life, to learning, and to medicine. Not only the place of our meeting, indeed, but some special circumstances of the present time seem to make it fitting that we should here review the progress, the proper scope, and the prospects of our chosen subject. We are now just half a century from the time when physiology first came to take its present position in this kingdom as one of the great branches of university learning and as a vital part of medical education. We have seen the close of a war which, though it diverted and distorted the progress of the science, yet brought it great opportunities of service in national life and taught us lessons, here as in so many other directions, of which we shall do well to take profit. The passing of the War, moreover, has brought a period of change and unrest during which impulses towards reform are being chequered by the results of fatigue or reaction. Both here and in America it may be said that, while physiology has come from the War with enlarged outlook and responsibilities, it is exposed to some new and perhaps dangerous influences in the present time of rapid resettlement. It may well be worth while, then, to look now both forward and back, to see the road by which we have hitherto been led and its relations to that which now lies before us.

I.

Physiology, as the passing generation has known it, took shape and established its boundaries in this country just fifty years ago, when, shaking off its long subordination to anatomy, it was brought to a new life of recognition and progress. The seventeenth century had seen England famous for her school of physiologists, leading the rest of the Continent in experimental results and in new ideas. Working upon the foundations laid by Harvey, that brilliant group at Oxford—Boyle, Lower, Mayow, Willis—had brought new light to the study of the living body. Nor was their service only recognised by fellow-workers abroad or by those that came after. Their names and fame were on fashionable lips; like that of their predecessor Harvey himself, under Charles I., and of that other Cambridge philosopher Glisson,

their immediate contemporary, their work was aided by the direct interest and favour of the sovereign. But, during the eighteenth century and the earlier part of the nineteenth, eclipse fell upon the light that had thus burned so brightly, though isolated gleams shone here and there. James Jurin, under George II., applied the Newtonian principles to calculating the work done by the heart and to other problems of the body, but his efforts to lay true and exact foundations for the study of disease were premature in the absence of experimental data. Stephen Hales, Chaplain to the future George III., made the first measurements of blood pressure in his garden at Teddington, and made many far-reaching observations of the first importance; but, as he wrote, there was indeed 'abundant room for many heads and hands to be employed in the work, for the wonderful and secret operations of Nature are so involved and intricate, so far out of the reach of our senses . . .'; and it was not then or till much later that many heads and hands were ready to be employed. Neither of these men had effective influence upon the thought or practical affairs of their day, either within the universities or outside them.

Physiology, as we know it now in this country, took its shape in a new revival which may be reckoned as beginning half a century ago. All our chief schools may be said to derive their lineage from that new home of active and unshackled inquiry—I mean University College, in Gower Street, London—and from the influence there of an Edinburgh graduate, William Sharpey, who at the age of thirty-four was taken from the Edinburgh school to be Professor of Anatomy and Physiology. Here, from 1836 until 1874, Sharpey was inspiring a group of younger minds with his eager outlook. Already in France the new experimental study of the living functions was being established by Claude Bernard—that true 'father in our common science,' as Foster later called him; already in Leipzig Ludwig, transmitting the impulse of Müller's earlier labours, had founded that school of physiology which moulded the development of the subject in Germany and other countries, and had very strong early influence upon several of those who were later to become leaders with us. England had lost the pre-eminence that Stuart kings at all events had valued and promoted. Learning had become identified in English society with the mimetic use of the dead languages, and progress at the two universities—even at the Cambridge of Newton, where mathematics kept independence of thought alive—was still impeded by the grip of ecclesiastical tradition and by sectarian privilege. But at University College learning had been unfettered. Here Sharpey and his colleagues were in touch with the best progress in France and Germany, and here the organised study of physiology as a true branch of university study may be said to have begun. Its formal separation from anatomy came later and irregularly; a separate Chair of Physiology was not created at University College until 1874, nor at Cambridge or at Oxford until 1883.

We ought in piety to recognise that this tardy reflection of Continental progress in our own subject, like parallel movements in other subjects, had in its early stages received invaluable aid from the Prince

Consort, who, familiar with the progress of other countries, had lent his influence and sympathy to many men of science in their struggle against the insularity and apathy of the wealthy and governing classes of the earlier Victorian days. The curious may take note that the first outward mark of recognition given by the official and influential world to the existence of physiology as such was given not, as in other and poorer countries much earlier, by the endowment of some chair or institute for research and teaching, but by an act of symbolic representation. For, when the expensive statuary of the Albert Memorial was completed in 1871, it was found that 'Physiology,' betokened by a female figure with a microscope, had been given its place among the primary divisions of learning and investigation acknowledged in that monument to the Prince.

From Sharpey himself and his personal influence we may trace directly onwards the development of all the chief British schools of physiology whose achievements have in the past half-century restored Britain to more than her old pride of place in this form of service to mankind. We here fittingly acknowledge first the close link with Sharpey which we find here to-day in Sir Edward Sharpey Schafer, who, after fruitful years in his old teacher's place at University College, brought that personal tradition back to this great school of Edinburgh from whence it originally came. At University College itself the line has been continued with undimmed lustre by Starling and Bayliss and their colleagues to the present day. From Sharpey's school again are derived the great branches which have sprung from it, both at Oxford and at Cambridge. Burdon Sanderson, Sharpey's immediate successor at University College, proceeded thence to Oxford and founded there, against many difficulties of prejudice and custom, the school of physiology which Gotch, Haldane, and Sherrington have nevertheless maintained so brilliantly in succeeding years. To Cambridge, Michael Foster, one of Sharpey's demonstrators, was invited in 1870 by Trinity College to be Praelector in Physiology and Fellow of the College. This enlightened and then almost unprecedented act, no less than the personal qualities of Foster that so aboundingly justified it, I would, as in private duty bound, hold here in special remembrance. Under Foster's influence there came into being at Cambridge a strong and rapidly growing school of physiologists, from Langley, Gaskell, Sherrington, Hopkins, to numerous successors. There sprang from him, too, a new impetus to other subjects, through his pupils Francis Balfour and Adam Sedgwick to embryology and zoology, through Vines and Francis Darwin to botany, through Roy to pathology. From Foster again through Newell Martin, who, coming with him from London, had caught not only inspiration from him but some of his power of inspiring others, and who left Cambridge for a Chair at Baltimore in 1876, we may derive a large part of the growth and direction of physiology since that time in the United States and in Canada. The rapid progress of all these biological sciences at Cambridge within a single generation, and the volume of original work poured forth depended, of course, upon two necessary conditions. The first is one which has never failed in this country—the existence of men fitted by temperament to advance

knowledge by experiment. The second has been the supply of living necessities through the ancient endowments of the colleges, and these in the Cambridge of the last half-century have been freely and increasingly used in catholic spirit for the increase of any of the borders of knowledge.

If these have been the chief lines of descent along which our present heritage has come to us, as mind has influenced mind and the light has been passed from hand to hand, what has been the outcome as we look back over the half-century to those small beginnings?

Truly we can say that the workers in this country have in that short space of years laid the whole world under a heavy debt. In whatever direction we look we seem to see that in nearly all the great primary fields of physiological knowledge the root ideas from which further growth is now springing are in great part British in origin, and based upon the work of British experimenters. If we consider the blood circulation we find that our essential ideas of the nature of the heart-beat were established by Gaskell, and that other first principles of its dynamics and of its regulation have been laid down by successors to him still with us; that the intricate nervous regulation of the arterial system has had its chief analyses here, and that here have been made more recently the first demonstrations of the part played by the minute capillary vessels in the regulation of the distribution and composition of the blood. Of the central nervous system the modern conceptions of function in terms of the purposive integration of diverse impulses along determined paths have sprung direct from British work, while the elementary analysis of the structure and functions of the sympathetic nervous system has been almost wholly British in idea and in detail. As with the nervous regulation of the body, so with the chemical regulation of function by travelling substances—the so-called 'hormones,' or stimulants from organ to organ—this, too, is a British conception enriched by numerous examples drawn from experimental work in this country. In the study of nutrition, of the primary 'foodstuffs,' proteins, carbohydrates, fats, salts, and water, whose names in their supposedly secure sufficiency were written with his own hand by Foster upon the blackboard shown in his portrait by Mr. John Collier, to typify, as we may imagine, a basal physiological truth, we have come to learn that these alone are not sufficient for growth and life in the absence of minimal amounts of accessory unknown and unstable substances, the so-called 'vitamins,' which are derived from pre-existent living matter. This conception, undreamt of to the end of the nineteenth century, has fundamental value in medicine and in agriculture, and has already begun to bear a harvest of practical fruit of which the end cannot be seen or the beneficence measured. This discovery stands to our national credit, and large parts of its development and application have been due to recent British work. If we turn to the regulation of respiration and its close adaptation to body needs, that also, as it is now known to the world, is known as British labours have revealed it, just as the finer analyses of the exchanges of gas between the air and the blood and between the blood and the body substance have been made with us. The actual modes by which

oxygen is used by the tissues of the body, its special relations to muscular contraction, the chemical results of that contraction, the thermal laws which it obeys—all these fundamental problems of living matter have seen the most significant steps to their solution taken within the past generation in this country.

Work of this kind brings permanent enrichment to the intellectual life of mankind by giving new and fuller conceptions of the nature of the living organism. That we may think is its highest function; but it does more than this. Just as all gains in the knowledge of Nature bring increase of power, so these discoveries of the past fifty years have their place in the fixed foundations upon which alone the science and the arts of medicine now or in the future can be securely based. The special study of disease, its cure and prevention, has had notable triumphs here and elsewhere in the same half-century, and these as they come must make as a rule a more spectacular appeal to the onlooker. Yet it is the accumulating knowledge of the basal laws of life and of the living organism to which alone we can look for the sure establishment either of the study of disease or of the applied sciences of medicine. As we have seen, there are few indeed among the fields of inquiry in the whole range of physiology in which the British contributions to the common stock of ascertained knowledge or of fertile idea do not take a foremost place. It would be impiety not to honour, as it would be stupidity to ignore, these plain facts, which, indeed, are now perhaps more commonly admitted abroad than recognised at home. There is no occasion here for any spirit of national complacency—rather the reverse, indeed. British workers at no time earlier than the War have had the menial assistance or other resources which their colleagues in other countries have commonly commanded, and too often the secondary and relatively easy developments of pioneer work done in this country have fallen to well-equipped and well-served workers elsewhere. If in the past half-century better support had been available from public or private sources, or at the older universities from college endowments, it is impossible for any well-informed person to doubt that a more extended, if not a more diversified, harvest would have been won.

We stand too near to this remarkable epoch of progress to appraise it fairly. In the same span of years Nature has yielded many fresh secrets in the physical world under cross-examination by new devices which have themselves been lately won by patient waiting upon her. So great a revelation of physical truth has been lately made in this country, bringing conceptions of space and of matter so swiftly changing and extending, that our eyes are easily dimmed to the wonders of that other new world being unfolded to us in the exploration of the living organism. Only the lapse of time can resolve the true values of this or that direction of inquiry, if indeed there be any true calculus of 'value' here at all. We seem to see in the progress of physiology, not at few but at many points, that we stand upon new paths just opening before us, which must certainly—as it seems—lead quickly to new light, to fuller vision, and to other paths beyond. The advances of the next half-century to come must far exceed and outshine those

due to the efforts of the half-century just closing; that is probably the personal conviction of us all. Yet we may still believe that through all the history of mankind recognition will be given and honour be paid to the steps in knowledge which were made first and made securely in the period we now review. The men who have done this work will not take pride in it for themselves; they know that their strength has not been their own, but that of the beauty which attracted them, and of the discipline which they obeyed. They count themselves happy to have found their favoured path. Other and more acute minds might have usurped their places and found greater happiness for themselves if, under a social ordering of another kind, they had been turned to the increase of knowledge instead of to the ephemeral, barren, or insoluble problems of convention and competition. By how much the realised progress towards truth and the power brought by truth might have been increased under a changed social organisation we can never know, nor can we guess what acceleration the future may bring to it if more of the best minds are set free within the State for work of this highest kind, what riches may be added to intellectual life, or what fuller service may be given to the practical affairs of man and to the merciful work of medicine.

II.

To the story of progress which has just been sketched in outline the War brought inevitable interruption and change. To the more obvious disturbances and wastage of war I need not here refer, but I would point to some influences of that time which will be found, I think, to have left permanent effects, and on the whole good effects, upon the position and tendencies of physiology. Before 1914 physiology was being developed, as we have seen, in its still youthful status as one of the primary departments of knowledge; it had become a subject of independent university rank. Large and important parts of this development had proceeded at one or other of the ancient universities, out of touch with great centres of population, and out of touch, therefore, with immediate medical needs. In some degree this was not without advantage, and for two main reasons. Detachment from the pressure of need allowed the free pursuit of knowledge for its own sake and a full surrender to the hintings of Nature, wherever her clues might lead the inquirer. Experience amply showed, moreover, that when physiology was presented among other university subjects for study it gained, first as recruits and later as distinguished workers, many able young men who were attracted to it, often from other subjects, by the fascinations of its problems, and without regard to any of its potential applications to medical or any other practical ends. These were great gains which it would be easy, if it were not unnecessary, to illustrate by many convincing examples. Yet there were some heavy counterweights on the other side of the balance. The practical and urgent needs of humanity as found at the hospitals were not brought with full or due effect to the notice of physiologists. Those in charge of hospital patients were not selected to advance, or habitually

engaged in advancing, medical knowledge, and new physiological conceptions as they took shape in our laboratories only slowly and partially came to have effect in medical practice and medical study. The physiologist, to his own certain loss and to the no less certain loss of medicine, held aloof from the bedside, often when access was possible, and remained immersed in his laboratory interests. Little pressure, indeed, was ever brought to bear upon him by the physician to come to his aid. Connected with the evils of this separation was the divorce which the accidents of development had set up between physiology and pathology, as though the study of the damaged body could be separated from the science of the living organism and of its reactions to any disturbance from the normal. Yet, while the physician had come to tolerate the approach of the pathologist to the bedside, it occurred too rarely that he felt the need of the physiologist, or made himself familiar with new devices of physiological investigation.

If from a hospital in time of peace the most obvious call had seemed in the past to come from the side of infective disease or morbid process for the help of the pathologist, in war the stresses put upon the healthy human body made the physiologist and his methods indispensable. Bacteriological work and studies of immunity had their prominent place, of course, in the detection and prevention of infective disease, and wonderful were many of the achievements seen under this head. But in a sense the more complete the prevention of infective disease the more apparent became the physical stresses of war. The violences offered in modern warfare to the human body—whether through exertion and exposure, by terror or excitement, in physical damage by lead or steel or in chemical attacks by poison, and not least through the incredible stresses of flying high and fighting in the air—all these brought many new and urgent calls for precise physiological knowledge and for new studies by the physiologist. The results of pain and fear, of hæmorrhage, of 'shock' by wound or operation—all these needed further analysis before sound treatment could be devised or improved. New studies were needed of changes in blood-pressure and blood-volume and in the qualities of the blood itself, new inquiries into the finer vessels of blood circulation and their relation to the nervous and other systems, and new analyses of the chemical mechanisms of the body and of the modes by which want of oxygen is met by adaptation or leads to final damage. But the well-nigh incredible demands made upon the machinery of man's body in and behind the battle-line, in all situations upon the land or under the earth, high in the upper air, in the sea or within its depths, by no means make up the tale. Our forces were engaged in every climate, from the Equator to the Arctic regions, and were faced by innumerable local or accidental variations of diet. Here again were required the applications of physiological studies of heat loss and of heat production to manifold practical problems of clothing and of diet. What would have seemed a fanciful fairy tale barely twenty years ago might in particular be told of the miracles wrought by the studied application of our new knowledge of 'vitamins' in diet, in saving from painful disease or death many thousands of men in diverse climates and fields of war.

At home the bodies of the civilian population were exposed to many stresses, often hardly less than those of active service. Men and women alike were exposed to arduous toil, to dangerous occupation, to poisons of many kinds needed for munitions, and in all these dangers the guidance of the physiologist was needed for the avoidance of industrial fatigue and loss of output and for devising protection against industrial poisoning. The whole nation was threatened by the menace of starvation, and our escape from that, itself one of the governing conditions of our ultimate victory, was due to a system of rationing and of the management of food materials, animal and vegetable, which was based on accurate physiological knowledge, won by experimental methods.

I touch on these points here briefly and in outline only in order to draw attention to the special influence which, as I think, the War has exercised upon the position of physiology in this country. The physiologists gave no exceptional help to the nation during the War; the exponents of every branch of science were needed, were ready, and were used, in our national crisis. Hardly one division of science can be named the deficiency of which would not have made defeat inevitable. It is a truism and a commonplace to say that no bravery and no fortitude could have avoided defeat without the help of scientific men and of the fruits of experimental science, though that commonplace has not, I think, ever yet been enshrined in the addresses or thanks of Parliament or in the prayers and thanksgivings of our churches. But we may recognise, perhaps, that the nation as a whole, and those especially who have the government, public or private, of large groups of men in their hands, have learned that obedience to physiological law is a first necessity for the maintenance of the body machinery in health and for its effective and harmonious use. They have come to know, moreover, that the men who alone can guide them to this obedience are those who have learned in the school of investigation from Nature herself. The nation has seen a Minister fall whose control of the people's food was not based upon physiological law, and his successor gain renown whose adoption of the teaching of physiological experiment was early and faithful. Nor was this by any means an isolated object-lesson. There is no doubt, surely, that physiologists have a new vista before them of immense public usefulness, if they will hold themselves in readiness to give the same kind of service to the country in the stress of her industrial life during peace as they gave so freely and to such effect in time of war.

But if the War brought these lessons to the general public, what lessons have come from it to the physiologist himself? I would only recall briefly here the considerations which were brought home with sufficient clearness to us all, I think, during and after the closing stages of the struggle. The War, in the first place, displayed before us new and gigantic fields of physiological study. Viewing these so far as we can, even at this distance, dispassionately, we see how the stresses and accidents of warfare in all their variety offered to our study a series of experiments made upon the human body, and on a gigantic scale. Only by disciplined study of the results at all stages of these trials of war in all their varying

degrees of horror and distress could effective aid be given in palliation of suffering or its avoidance. It was inevitable that study of this kind and upon so great a scale should result—as, happily, it did result—in much permanent gain to physiological knowledge and to the beneficent power that all sound knowledge brings. New insight was given into the functional patterns of the nervous system and into the orderly hierarchies, so to speak, under which this or that function is brought into subordination to another of superior rank, and new knowledge was gained of the phenomena of separation and repair in the outlying nerve-trunks. Accurate information was collected of the nutritional needs, quantitative or qualitative, of human beings under varying conditions; and, in particular, many special conditions of warfare brought to the test, established the fundamental usefulness, and stimulated the growth of that newest chapter in physiology already mentioned—that dealing with the elusive but potent accessory factors in nutrition—the vitamins. These examples must suffice where scores of others familiar to all of you might be given.

In the second place, the experience of the War has had wholesome effect from its tendency to remove the barriers that here and there had grown up between physiologists and the practical needs of medicine. Physiologists had valued, and justly valued, their academic freedom of inquiry within the universities, and, indeed, we know that practical utility could not be better served in the long run than by the detached pursuit of knowledge for its own sake. But, partly for reasons of hospital and professional organisation already touched upon, and partly because, to its obvious and immense gain, physiology had attracted from other paths men who were not, and had never become, medical men, there were some capital parts of the subject of which the chief explorers had never used the medical field of work or brought to medicine the weapons they had, perhaps unwittingly, at command. We can recognise already that this partial divorce has been changed by the War into a union likely to be increasingly fertile. Of the professorial chairs of medicine or directorships of medical units established since the War, for the advance of medical knowledge within hospitals in accordance with the university standards and ideals acknowledged in other subjects of study, it is remarkable that to the greater number of these there have already been appointed men whose training has been in the methods of the physiological laboratory, and who applied that training to urgent medical problems of the War. There is hardly any one of our schools of physiology, moreover, to which some piece of living experience has not been brought in these last years to enforce the old lesson of the value to science itself of bringing natural knowledge to its fullest utilitarian applications. The practical fruits of scientific labour are found, if our hands are put out to gather them, to contain within themselves, like the natural fruits of the earth, the very seeds from which new knowledge and new fertility will spring. Many of our leaders in physiology brought to the problems of war the accumulated knowledge of their lives, as patriotism and humanity dissolved at a touch the hedges of custom and use. I know of not one such who did not find in the application of his vision and

training to the actual problems before him, first, a wholesome reminder of the limits of his knowledge and its clarity, and, second, new clues towards its advance, and that by no means only in a familiar or an expected direction. The stimulus of practical need here, as so often in experience, advanced the growth of knowledge beyond the point of immediate application to practice. Those who studied to find the best and most practical means of saving life threatened by severe hæmorrhage, or by the shock of wounds or operation, found in the course of meeting the immediate emergencies almost endless promptings to further inquiries, to be followed then or later—inquiries into the physical, chemical, or biological qualities of the blood, into its relations to the vessel walls, and into the functional changes of the capillary blood system and the factors affecting or controlling them. Those who fixed their attention upon the damage wrought in the respiratory organs by poison gases were led to many new studies of the fundamental physiology of the lungs. The lymphatic system of drainage of the lungs was re-examined, and wide new experimental studies of the modes of regulation of the breathing were undertaken which have thrown new and valuable light upon the normal mechanisms of respiration. An inquiry into the poisonous action of the high-explosive trinitrotoluene, and into the possibility that slightly abnormal forms of this substance, found as a small contamination of the normal form, might be specially toxic, led to a clear negative answer. But it led unexpectedly, it is both curious and useful to note, to the discovery that one of these abnormal forms was an effective reagent in the laboratory. By its means the chemical structure of a constituent of muscle substance known as carnosin was for the first time determined, and carnosin has now been synthesised artificially from simple materials.

In sum, then, we may gratefully recognise that the War in its horror and waste has not brought evil without any admixture at all of good. We may be encouraged at least to hope that the active co-operation which the War established and fostered in diverse ways between the physiologist and the medical or surgical clinician may remain to bring lasting good, on the one side to the cause of learning and its advance, and on the other to medical education and to medical progress.

III.

If we have thus looked backward to the development of physiology in the past half-century, and to the influence upon its course which the War has brought about, I would invite you to look forward to the future and to review the aims of physiology and the boundaries to which it should properly extend in its relations to other subjects of study.

Foster, early in his work at Cambridge, spoke of physiology as being the study of the differences between the living body and the dead body. The progress of this study, as it has been carried on during the past generation, may be considered from two directly opposite points of view. Viewed in one way, we may think of this progress as being a progress in analysis, as a disentanglement of the diverse though not separable functions of the body and of each of its parts. Viewed

again, we may see it as a steady progress towards synthesis, towards the unification of all the contributory functions of the parts into a single functional organism.

The analysis of the separate functions of each part of the body was an inevitable mental process as the anatomist revealed more and more accurately the visible machinery of the body. Bichat at the beginning of the nineteenth century had taught that the activities of the body must be the sum of the activities of the organs. The announcement of the universal cellular structure of the organs made by Schwann seemed but to carry this analysis one step further, and to show that in the sum of the activities of the constituent cells could be found the adequate expression of the functions of the whole body. The rapid improvement of the microscope in the latter half of last century, combined with the new resources of the aniline dyes by which transparent structures could be differentiated and made visible, greatly stimulated the analytic study of the body. As the various glandular structures were made visible and even, as it almost seemed, the inner life of the gland cell was revealed, as muscle fibres in their different kinds were made plain and the harder elements of the body resolved into the architectures due to different kinds of constructive cell, so it seemed to many that in a little more we should have the quest resolved in an appeal to a congeries of physico-chemical events within the individual cells. Even the mysteries of the central nervous system seemed to be dissolving as the new powers of histology, coupled with refined methods of experiment, showed the intricate pattern of communicating fibre and cell and gave provisional descriptive explanations of many isolated nervous phenomena. Meanwhile, the chemical structure, no less than the material form, of the body was being explored, and here, too, progress followed the path of analysis, ever more refined and complete. Just as old notions of 'humours' of the body had been resolved into varieties of cell activity, so the vague chemical ideas conveyed in the words 'protoplasm' or 'metabolism' received precision by expression in terms of colloidal systems or of associated enzymes or catalysts in appropriate positions, effecting chemical changes of recognisable type among substances of relative simplicity.

Along these lines of analysis rapid progress has been made, but it is to be observed that it has been in great part along diverging lines. The tendency has been centrifugal, or, to use a biological simile, the growth of physiology has led to a fissiparous habit. Pursuit of knowledge by particular technical methods has led to specialism; men have reached points far distant along branches of inquiry that at first grew together from the stem. The very development of new technical methods may by itself lead unavoidably to separatism, for the microscope and test-tube may best be used in rooms widely different in equipment and often far separated in space. So have grown up new-named sciences within a science, and the histologist or cytologist, the neurologist, the pharmacologist, the biochemist—each carrying off, so to speak, his part of the subject—may be found to be incurring the dangers or even paying the penalties of schism.

Step by step, however, with this progress in analysis, a continual

advance towards synthesis has accompanied it as new truths have been unfolded to the investigator. Here, as in other fields, the conception that the whole is the same as the sum of its parts is either meaningless, or, if it have any meaning, is untrue. Fresh reinforcements have steadily come to the idea that the animal body is not to be rightly considered as a patchwork of the activities of its parts, but that the organism itself as a whole is the true physiological unit. In this conception the functions of the organs and of their own cellular subdivisions can only find due expression in relation to each other and to the functions of the whole. Just in proportion as analysis has proceeded with ever greater refinement to trace in terms of physics or chemistry the nature of given organic or cellular phenomena, the analysis itself is found to be pointing to new relationships between part and part of which the meaning is bound up in the unity of the organism.

Of this continued absorption of analytic data into synthetic conception, this interweaving of increasingly manifest diversities into an increasingly emergent unity, illustration can be found in many directions. The name 'hormone' has been given to chemical products of particular organs which pass by way of the blood to stimulate another organ or other organs of the body to changes in activity. This mode of chemical regulation by messenger, so to speak, is superadded to the more rapid method of regulation by nervous impulse through the nervous system: and already many beautiful examples of delicate interplay and co-ordination have been discovered between the two kinds of regulation. In its earlier phases the knowledge of these messengers gave us a picture of relatively simple, though wonderfully adjusted, acts of chemical regulation. As analysis of the hormonic exchanges of other glands and tissues of the body has proceeded, however, a system of interplay and reciprocal function of increasing complexity has been revealed by later studies. Our knowledge of this is still young and quite rudimentary, but at every fresh step in this advance it becomes more evident that the multiplying facts can only be resumed by a conception of the whole organism as a unit of which the parts exist to preserve the integrity and 'normality.'

In the study of the nervous system, again, new methods of observation and analysis have given us during the past half-century immense additions to our knowledge of the intricate fabrics of the brain and spinal cord and of the functions of the various systems of fibres and cells. The content of our knowledge of these must be tenfold that which was known fifty years ago. Here again, as investigation has gone forward, and as analysis has proceeded by methods so special and so refined that neurologists work, as it were, in a field of their own, it has proceeded only to reveal ever more and more clearly what Sherrington, one of the chief pioneers in this analysis, has himself called the 'integrative' action of the nervous system. The fabric of nerve cell and fibre, whether we trace its history from the lower to the higher animals, or whether we trace its complexity in the individual, is revealed to us as a series of superimposed controlling systems whose structural relations find intelligible expression only in terms of functions, and of functions of the animal as a whole.

Is the same return to synthetic conceptions to be found as a result of analyses of the biochemist? His work has brought much simplification to our notions of the chemistry of the body. We have learned that in the exchanges within the living cell we are not necessarily, or indeed probably, dealing with molecules of a complexity unknown outside the living body; we do not now think as formerly of substances being worked up through successive stages of elaboration into a living molecule—a molecule of 'protoplasm' of mystical complexity—or of other substances reappearing as the result of incessant degradation of parts of the living molecule. Analysis has shown already that many characteristic cell-changes turn upon relatively simple reactions of a kind familiar in chemistry between known and relatively simple substances. How much further will this analysis proceed? No doubt many of the typical functions of particular kinds of cell will become expressible in a set of chemical formulæ, and every simplification attained by the biochemist in terms of known chemical or physical law will be a notable gain. Yet even now we can feel assured that the analyses of the biochemist bring with them new emphasis upon the essential unity of the whole organism. Let me give but one illustration of this. In the studies of immunity from disease it had long been known that substances which to a chemist would appear to be identical could be sharply distinguished in the most decisive way by biological reactions. Tiny fragments of a small blood-clot can be made thus to declare whether they come from a man or from what other animal, when no chemist would have dreamed of finding a distinction. Dudley and Woodman have lately been able, however, to bring biochemistry within the range of this biological delicacy of discrimination, and have shown a subtle difference in the chemical architectures of the caseins derived respectively from the milk of a cow and of a sheep. More recently the two modes of analysis have been brought side by side. Similar cells in similar organs of the two not widely dissimilar birds, the hen and the duck, secrete layers of egg-albumin during the completion of that wonderful structure, the egg. From the 'white' of each egg can be prepared apparently identical albumins, and in a pure crystalline form. This albumin is built up in each case from simple materials—amino-acids—derived from the food, and we should naturally expect a close similarity between the two kinds of resulting albumin, that in the hen's egg and that in the duck's. The most refined methods of ordinary chemical examination show us, indeed, that the two are chemically identical and indistinguishable, containing on analysis the same amounts of the same varieties of amino-acids. But Dakin has lately succeeded in tracing a difference between the two albumins, exhibited only as partial differences in the order or pattern in which some of the constituent amino-acids are linked together in the structure of the albumin molecule. By using a physiological test, Dale, at the same time, has been able to show a decisive and even dramatic difference between the qualities of the two albumins so near to chemical identity. By using the 'anaphylactic' reaction of the organic tissue from an animal 'sensitised' against hen albumin, he has found that a suitable application of hen egg-albumin will produce a decisive response,

while an exactly similar dose of duck egg-albumin will produce no effect whatever; and so *vice versa*. Here, then, is some authentic stamp of unknown kind imposed uniformly upon the parts of the organism of a given species, even upon the molecules of the albuminous coating of its egg. We are brought sharply back from the relative simplicities of chemical analysis to consider this supra-chemical impress of specific pattern, a phenomenon which can have no meaning that is not drawn from a conception of the organism as a whole.

It would be impossible here, and quite unnecessary for the present purpose, to do more than refer finally to the beautiful researches of recent years upon the modes of regulation of breathing, upon the gas exchanges of the blood, and upon the associated activities of other organs, and especially of the kidney, which have brought such ample support and illustration to the doctrine first clearly taught by Claude Bernard, namely, that the different mechanisms of the body, various as they are, have their single object in 'preserving constant the conditions of life in the internal environment.' These regulative functions in particular have been fully discussed by Dr. Haldane in a recent notable essay, and he has shown how, as their chemical analysis has proceeded and observations have been collected by physiological methods, themselves of a delicacy often far exceeding those of present physical and chemical methods, it has become more and more necessary to express the facts in terms of an organic unity. 'The physical and chemical picture is entirely obliterated by the picture of organism.' These considerations are full of interest, of course, in their relation to the rival mechanistic and vitalistic theories that have been advanced for the explanation of living processes. Here, however, I refer to this synthetic tendency of modern physiology because of its practical bearing upon the present development of the subject in the universities and the medical schools. As the preliminary analyses of the functions have been, as we have seen, centrifugal and fissiparous in their tendencies, so the accompanying and inevitable synthesis, resuming analytical data within the notion of organism, has been centripetal and conjugative. It is this bond of organic unity which must sooner or later serve to bring together the scattered workers in different fields of analysis. It is this conception of the organism, moreover, which must maintain physiology as a great primary branch of study—the study of the living organism.

If physiology remains as a free subject of university study, we need not have serious fear that the fissiparous, centrifugal tendencies already noticed will be dangerous or crippling. Ludwig organised his physiology teaching at Leipzig in 1846 under the three main divisions of histology, experimental work, and physiological chemistry. In the English revival that we have earlier sketched, this grouping, largely under the influence of Foster, was maintained not only at Cambridge, but at other centres here and in America. As years have passed, however, there has been an increasing tendency here to follow what is commonly done in other countries, and to place histology with anatomy. In my personal view, physiology cannot proceed without perpetual use of the microscope, and yet anatomy must be dead without histology. I should hope to see histology the well-worn bridge of union between the

two subjects, just as, I think, we should look to cytology and the study of cell development to offer active points of growing union between physiology and the sciences of animal and plant morphology. These and other questions of detailed organisation will, I hope, be explored fully in the discussion for which we are hoping to-day. With time also has come a great development of biochemistry, and this, if only from the structural necessities of its laboratory technique, is tending more and more to set up house for itself. This, too, is to be a matter for fuller discussion presently. We may perhaps hope to see in biochemistry as it grows not only a common meeting-ground and an unfailing source of new inspiration for physiologists and pathologists alike, but also a pathway by which organic chemistry may be led towards the study of living matter. Few organic chemists in this country, though more in America, have been led by that path till now, and yet we must believe that biochemistry has perhaps even more to give to organic chemistry, as we now know it, than it has to gain. A study of organic compounds in a spirit of detachment from the living processes which gave them birth must surely lead often to mere virtuosity in the laboratory transformations of chemical structure, and I venture very timidly to think that many signs point to the near approach of a time when organic chemistry will feel the need of fresh inspiration coming from the intricate laboratory of the living cell. In a university the separation of laboratories, which must be guided solely by convenience, as convenience is dictated by necessary differences in equipment and technique, may be easily transcended by the free communication of workers in different branches. Intellectual association and close co-operation, and especially within a university, seem inevitable, as we have seen, because of the converging approach of diverse workers in common reference to the conception of organic unity. There can be no boundaries to physiology narrower than the limits of the study of the whole organism and the balanced regulation of its living parts.

I would venture here, however, to point to some dangers by which the sound development of physiology seems to be threatened, that spring from its necessarily close association with medical education, dangers eminent in the present stage of rapid growth in medical studies both here and, even more obviously, in America. Historically, physiology may be said to have been born of medicine, but it has sanctions and a strength quite independent of the great services it has rendered and has still to render to the material good of mankind through medicine, and, in a less, though in no insignificant degree, to agriculture. We may recall that chemistry, too, was almost equally born of medicine; medicine, at least, was the foster-mother and long the nurse of chemistry. Lyon Playfair, in his inaugural address of 1858, in this very place, said, nevertheless, that 'chemistry in her period of youth, full of bloom and promise, was forced into a premature and ill-assorted union with medicine.' We can now look back and see that chemistry, in becoming free of medicine, and in becoming a great independent branch of learning, has, by the fruits of that freedom, repaid to medicine a thousandfold her early debts of the nursery. So, too, the history of the last half-century, in which physiology has become an independent

subject of university study, shows how this freedom has multiplied the gifts which physiology has had it in her power to return to her ancient mother. There can be no dissolving of the ties between one and the other, but we must see to it that these ties are well adjusted and that there shall be no 'ill-assorted union' between the two.

In the rapid growth of medical schools throughout the English-speaking world there are present signs that the essential part which physiology plays in medical education and study may wrongly masquerade as the only service physiology has to give to man, and may appear to fill the measure of her rightful status. In more than one of the great American universities physiology is treated either in theory or in practice as a subject within the Medical Faculty to be housed within the Medical School, yet at the same time not as a subject in the Faculty of Arts or of Science, nor to be studied alone or with other sciences as part of a liberal and non-professional education. It is rare in the United States for physiology to be studied by any but professed medical students, and there is some reason to think that it is becoming rarer in Great Britain than it was a few years ago.

To my mind this tendency is to be deplored. It implies a reversal of that growth of physiology in freedom which began half a century ago and from which such good fruit has already been gathered. It has two chief evils among its inevitable results. Removed from its position among other university subjects by geographical separation that in some universities amounts to transportation and exile, it is deprived of the kinship and co-operation of the sciences touching its own boundaries—those of zoology, embryology, and botany, of agriculture, of psychology, of physics and chemistry. Assigned, if not limited, to a place in the medical curriculum, it is apt to be narrowed in its claims and outlook, and to lose not only its proper neighbours, but even parts of itself, whittled away in the organisation of a purely medical programme in the guise of pharmacology, neurology, toxicology, and the like, for which special funds may be available, separate places in the time-table reserved, and independent departments provided. But a second evil strikes more deeply. Any arrangements that give in effect a restriction of physiological studies to medical students alone must be doubly injurious. It is injurious to the general course of education, because it tends to cut away from the other university students the opportunity of possessing themselves, either as a primary or secondary study, of the knowledge and discipline of physiology which has educative value in the highest degree for the cultural or the practical sides of living. And here, secondarily, we may notice the loss to an applied study only less in importance to that of medicine; I mean the science and practice of agriculture. It is injurious, again, to physiology itself, because we know well from reiterated experience how many promising recruits for the future advancement of the subject have been brought to it, often, as it were, by chance, in the course of their university life, attracted to it whether from classical studies or mathematical, or from other branches of natural science. A notable number of the chief leaders in the science of the past and present generation have so been attracted, without any previous thought of medical studies as such,

whether these have been added later or not; of these, not a few whose names are well known to us all have never become, in the technical sense, students of medicine at all. They may have lost by this, but should we willingly have lost them?

I hope that what I have earlier said with regard to the great service that physiology has both to give to medicine and to receive from it will acquit me of any charge of desiring less, rather than much more, intimacy and intercourse between them. I believe that no better service can be done for the good of both than to increase their mutual offices and the ties between them. But we must see that, in uniting physiology to medicine, we do not uproot it from that soil in which alone it can abundantly flourish and bear fruit, the environment of a university with all that that connotes. If there be any serious doubt of the reality of the dangers I have indicated, I would point to the dearth of men fitted to promote and teach the subject among those coming from the schools in which physiology is regarded as a medical study and no more, and is not given its full university status. In the United States at present there is a grave and admitted dearth of suitable candidates for chairs of physiology, in spite of the remarkable work which has been done there in recent years and the fine material equipment in general available. I venture to offer my conviction that the prime cause of this shortage is the absence of the great recruiting possibilities of university life and the undue limitation of physiology to medical students. Men coming to physiology as a 'preliminary subject' and nothing more are not likely to think of it as their life-work, but will pass through it not to return.

Let me, in conclusion, point again to the highest of the tasks which physiology, like every other science, has to perform. Its highest and indeed its primary task is to enlarge the vision of man and to enrich his knowledge of truth. The secondary tasks of physiology in finding power through truth, power to diminish pain and to restore health, and to guide to right and prosperous living, are happily so beneficent in kind, and already in some degree so fruitfully discharged, that it is not easy, or indeed common, to keep in mind that great and primary aim. Right thinking in this respect is the only constant guide to right action in all the practical questions which confront us now in our discussion of the position and the future of this science. 'Man does not live by bread alone': and we shall find—we have already abundantly found in experience—that it is only through the seeking of wisdom first that power to increase the comfort and convenience of life is most fully to be won. The practical services of inquiry have been easy for all to see. Men have come readily to think of physiology as the handmaid of medicine and as nothing more. Of late years we who follow the study of living things have not had interpreters to make plain to men at large the interest and beauty of the additions to revealed truth which have been coming from the work of the investigator. There are very few among the onlookers who have seen, or who can bring others to see, those clearer visions of the consummate beauty which are being revealed in the study of the body, visions as remote from the actual figments

daily painted for us by our sense organs as are the newest visions of the physical world, yet appealing as strongly to the intellectual and æsthetic emotions. Few hold the quest for natural knowledge in right relation to other activities of the mind; few see it not merely and not in chief as a useful pursuit of power, but in its essence as a pursuit of truth.

That knowledge of natural truth and of the changing pattern of our ideas of the natural world should be an unusual or quite subordinate part of a cultural equipment, in this and in recent generations, may be due to lack of interpreters, but it is due also to convention and educational habit, and these, perhaps, combine in special degree to shut out from the world of general culture the revelations of intricate beauty in the living body of man. Ancient and mistaken theological conceptions filtering through the Victorian age have tended to degrade the dignity and marvel of the body. Generations that have been nurtured upon narrowed classical studies have so far forgotten the spirit of Greece as to ignore the universal beauty of truth; it has been thought vulgar not to know the verbal details of an old mythology, but hardly respectable not to be ignorant of the elementary laws of life and of the unseen beauties of the body unfolded in modern study. So have many submitted to be enchained in ignorance and superstition as to vital matters of reality, victims of every passing charlatan. Out of this loss of instruction in the beauties and wonders of living substance, as they are becoming known, must come great loss of possible happiness, and indeed there comes, too, a loss of dignity, for we may fitly apply the rebuke of Robert Boyle, much more deserved now than in his darker century, who held it to be 'highly dishonourable for a Reasonable Soul to live in so Divinely built a Mansion, as the Body she resides in, altogether unacquainted with the exquisite Structure of it.'

Meanwhile the workers will proceed in their quest for further truth, caring little if, for the time being, other eyes are blind to its beauty. They will still be lured by it as all eager minds have been lured before; some will confess the attraction of a call for help in human need and suffering, some will claim austerely that they follow only the bidding of a curiosity of mind, and some perhaps may work for fame. But, whether they know it or not, the effective lure that Nature holds out to those of her followers who have it within them to respond to it, and so to reach new knowledge, is a quickening hint of further beauty to be unfolded in further truth. Whether they know it or not, they might make the same Confession as that of St. Augustine: 'And I replied unto all those things which encompass the door of my flesh, "Ye have told me of my God, that ye are not He: tell me something of Him." And they cried, all with a great voice, "He made us." My questioning them was my mind's desire, and their Beauty was their answer.'

CONSCIOUSNESS AND THE UNCONSCIOUS.

ADDRESS BY

C. LLOYD MORGAN, LL.D., D.Sc., F.R.S.,

PRESIDENT OF THE SECTION.

Psychology has now been given full sectional status, taking effect at this meeting of the British Association. I trust that we shall justify the confidence reposed in us by our fellow-workers in other branches of science. I need hardly add that I deem it no mean honour to be chosen as your President on this occasion.

The subject of my address bristles with difficulties. I may at once state that my primary aim is to consider in what way mind and consciousness may be regarded as natural products of that all-embracing process which I propose to name 'emergent evolution,' and thus come within the purview of science as I understand its aim and methods.

Emergent Evolution.

What do I mean by emergent evolution? Shall we start from the platform of that which we call common-sense as tempered by the refinement of scientific thought? By general consent we live in a world in which there seems to be an orderly passage of events. That orderly passage of events, in so far as something new comes on to the scene of nature, is what I here mean by evolution. If nothing really new emerges—if there be only permutations of what was pre-existent (permutations predictable in advance by some Laplacian calculator)—then, so far, there is no evolution, though there may be progress through survival and spread on the one hand and elimination on the other. Under nature is to be included the plan, expressive of natural law, on which all events (including mental events) run their course.

From the point of view of a philosophy based on science our aim is to interpret the natural plan of evolution, and this is to be loyally accepted just as we find it. The most resolute modern attempt to interpret evolution from this point of view is that of Professor S. Alexander in his 'Space, Time, and Deity.' He starts from the world of common sense and science as it seems to be given for thought to interpret. In order to get at the very foundation of nature he bids us think out of it all that can possibly be excluded short of the utter annihilation of events. That gives us a world of ultimate or basal events in purely spatial and temporal relations. This he calls 'space-time,' inseparably hyphened throughout Nature. From this is evolved matter, with its primary and, at a later stage of development, its secondary qualities. Here new relations, other than those which are only spatio-temporal, supervene. Later in logical and historical sequence comes life, a new quality of certain systems of matter in

motion, involving or expressing new relations thus far not in being. Then within this organic matrix, already 'qualitied' (as he says) by life, there arises the quality of consciousness, the highest that we know. What may lie beyond this in Mr. Alexander's scheme may be learnt from his book.

This thumb-nail sketch can do slight justice to a theme worked out in elaborate detail on a large canvas. The treatment purports to formulate the whole natural plan of progressive evolution. From the bosom of space-time emerge the inorganic, the organic, the conscious, and, perchance, something beyond. And with this successive emergence of new qualities goes the progressive emergence of new orders and modes of relatedness. The plan of evolution shows successively higher and richer developments.

Such a doctrine, philosophical in range but scientific in spirit, to which, I may perhaps be allowed to say, I, too, have been led by a rather different route—I call emergent evolution.

The concept of emergence is dealt with by J. S. Mill, in his 'Logic,' under the consideration of 'heteropathic laws.' The word 'emergent,' as contrasted with 'resultant,' was suggested by G. H. Lewes in his 'Problems of Life and Mind.' When oxygen, having certain properties, combines with hydrogen having other properties, there is formed water, some of the properties of which are quite different. The weight of the compound is an additive *resultant*, and can be calculated before the event. Sundry other properties are constitutive *emergents*, which could not be predicted in advance of any existent example of combination. Of course, when we have learnt what happens in 'this' particular instance under 'these' circumstances, we can predict what will happen in 'that' like instance under similar circumstances. We have learnt something of the natural plan of evolution. We may also predict on the basis of analogy as we learn to grasp more adequately the natural order or plan of events. But could we predict what will happen prior to *any* given instance—i.e. prior to the development of this stage of the evolutionary plan? Could we predict life from the plane of the inorganic, or consciousness from the plane of life? In accordance with the principles of emergent evolution we could not do so. The Laplacian calculator is here out of court.

This is not the place to adduce the many facts at the inorganic stage of evolution, which, as I think, exemplify emergence (in this technical sense) with its hall-mark of something new, and its saltatory form of continuity—saltatory because there is often an apparent jump from one relatively stable product to another; continuous because there is no unfilled hiatus in the course of events. It is exemplified, as I think, in the modern story of the so-called chemical elements, in the very structure of the Mendeléeff table, in the systems of crystallography, and so on. In organic evolution it is recognised (though not under this name) by some biologists in the acceptance of mutations, in the outcome of much Mendelian research, and in the clue it affords to the origin of variations.

More to our present purpose, however, is its explicit recognition by Wundt in his advocacy of 'a principle of creative resultants' (Lewes

would have said 'emergents'), which 'attempts to state the fact that in all psychical combinations the product is not a mere sum of the separate elements that compose such combinations, but that it represents a new creation.' Clearly there is here emergence. But Wundt accepted the philosophy of what may be distinguished as 'creative evolution'—that which Professor Bergson in different form so brilliantly advocates. Wherein lies the difference? For M. Bergson the philosophical question is: What makes emergents emerge? Rightly or wrongly, I do not regard this question as one with which science, as such, is concerned; and in some passages at any rate this is the opinion of M. Bergson himself. Philosophy, he says, ought to follow and supplement science, 'in order to superpose on scientific truth a knowledge of another kind, which may be called metaphysical.' Be that as it may, his answer to the question: What makes emergents emerge? is Mind or Spirit as Vital Impulsion. (I use capital letters for concepts of this order.) Whereas, then, for Mr. Alexander mind as consciousness is an empirical quality emergent in nature at an assignable stage of evolution, for M. Bergson Mind, as Spirit, is the metempirical Source (I adopt Lewes's adjective) through the Agency of which emergent evolution has empirical being. For the one consciousness is a product of emergent evolution; for the other emergent evolution is the product of Spiritual Activity, which is sometimes spoken of as Consciousness. The methods of approach, the treatment, and the conclusions reached, are different. Although my present concern is with the former, this must not be taken to imply a denial of Spiritual Activity. Its discussion, however, belongs to a different universe of discourse.

In Mind.

To come to closer quarters with our sectional topic, what do we mean when we say that this or that is 'in mind'? In a well-known passage Berkeley distinguished that which is in mind 'by way of attribute' from that which is in mind 'by way of idea.' Fully realising that this should be read in the light of Berkeley's adherence to the Creative concept, one may none the less claim for it validity on the empirical plane where mind is regarded as a product of emergent evolution. The former, therefore (i.e. what is present in mind by way of attribute), I shall speak of as *minding*, the latter as that which is mind*ed*. The former is a character constitutive of the mind—that in virtue of which it is a mind; the latter as objective *to* the mind or *for* the mind. That which is minded always implies minding; but it does not necessarily follow that minding implies something minded.

Let me name a few of the many cases in our own life where not only does the minded imply minding (which always holds good), but where minding implies something definitely minded (which often holds good). Perceiving implies something perceived; remembering, something remembered; imaging, something imaged; thinking, something thought of; believing, something believed; and so on through a long list. In each case what I may call, in general, the *-ing* has, as its correlative, a more or less definite *-ed*. Whether correlative to unconscious mind-

ing, there is something more or less definite which is unconsciously minded, it is very difficult to say. But if I do not misinterpret current opinion it is commonly held that something minded is often present to, or for, the unconscious mind. I shall say somewhat more on this head later on.

The distinction based on that drawn by Berkeley may be expressed in another way. One may be said to be conscious *in* perceiving, remembering, and, at large, minding; that which is perceived, remembered, or minded is what one is conscious *of*. I am conscious *in* attending to the rhythm or the thought of a poem; I am conscious *of* that to which I so attend. I need not *then* be conscious *of* attending to the poem, though perhaps I may, in psychological mood, subsequently make the preceding process of attention an object of thought. I am well aware that Professor Strong has urged that, in its original use, the expression 'conscious of' was applied only with reference to mental process as such. One need not discuss this point. It must suffice to make clear the usage I accept.

Even in our own life there are cases in which one's consciousness *in* some experience—e.g. feeling fit or depressed—does not seem to have, correlative to it, anything definite of which one is conscious. It may, of course, be said that what one is here conscious of is some bodily condition, or some more abstract concept of welfare or the reverse. But, without denying that it may come to be so interpreted in reflective thought, it is questionable whether the dog or the little child knows enough about 'the body' or of 'welfare' to justify us in regarding these as objectively minded. There can be little question, however, that the dog or the child (and we, too, in naïve unreflective mood) may be conscious *in* such current episodes of daily life. Whether, therefore, there be something definitely minded or not, the emphasis is on minding (in a broad and comprehensive sense) as an inalienable attribute of that kind of being which we name 'mental.'

Mr. Alexander emphasises the distinction between what I have called the *-ing* and the *-ed* in the most drastic manner. He speaks of all that is in any way objective to minding as non-mental. I cannot follow his lead in this matter, because I need the word in what is for me (but not for him) a different sense. But what does he mean? It is pretty obvious that while seeing is a mental process in which I am conscious, the lamp that I see is not a mental process, but an object of which I am conscious. If, however, I picture the Corcovado beyond the waters of Rio Bay, is that mental? The picturing of a remembered scene is a mental process; but that which is thus pictured is not mental in the same sense. It is just as much re-presented for the remembering as the lamp is presented as an object for the seeing. And suppose I try to think of the four-dimensional space-time framework conceived by Minkowski; the thinking is unquestionably mental, but the framework thought of is not mental in the same sense. What is not mental in that sense Mr. Alexander calls 'non-mental.' I speak of that which is not mental in this sense as 'objective.'

A wider issue is here involved. Are we to include 'in mind' processes of minding only, or also that which is objectively given as

minded? Is the science of psychology concerned only with mental processes of the *-ing* order; or is it concerned also with all manner of objective *-eds?* One must choose. So long as we are careful to distinguish the *-ed* from the *-ing* it is better, I think, to include both.

Dependence and Correlation.

On these terms what is minded is no less mental than the process of minding. But I suggest that the word 'consciousness' should be reserved for that which Berkeley spoke of as 'in mind by way of attribute,' or, in Mr. Alexander's way of putting it, as 'a quality' of that organism which is conscious in minding. Anyhow, consciousness is here in the world. Creative Evolution says: Yes, here in the world, but not of the world. It acts (as *élan vital*) into or through the organism regarded as a physical system; but its Source is a disparate order of Being to which, in and for itself, and *an sich*, it properly belongs. It depends on the physical organism in act but not in Being. Now this, I urge, is a metempirical explanation of given facts, but not an empirical interpretation of them as (in my view) science tries to interpret. And its cause should be tried before a different court of appeal from that of science. Hence under emergent evolution one uses the word 'dependence' in another sense, and urges that the very being of consciousness, as a quality of the organism, depends upon (or implies the presence of) the quality of life as prior in the natural order of emergence. If we enumerate successive stages, then consciousness is a quality (4) of certain things (very complex and highly organised things) in this world. In these same things there is also present the quality of life (3), and a specially differentiated chemical constitution (2). Empirically we never find (4) without (3), nor (3) without (2); and we express this by saying that consciousness depends on (or implies the presence of) life; and that life depends on a specialised kind of chemical constitution. It is an irreversible order of dependence. But there are things, such as plants, in which we find (as is commonly held) life without consciousness; and other things, such as minerals, in which there is chemical constitution (not, of course 'the same' chemical constitution) without life. Furthermore, there seems to have been a time when consciousness had not yet been evolved; and an earlier time at which life had no existence. But this or that chemical constitution is itself an emergent quality (2) of certain things; and there was probably a yet earlier stage of evolution at which even this quality had not yet emerged—a purely physical stage (1) at which (let us say) electrons afforded the ultimate terms in relation within physical events, continuously changing under electromagnetic (and, of course, also under spatio-temporal) relations. That is as far as I, with my limited powers of speculative vision, can probe. Mr. Alexander, with perhaps more piercing insight, goes further. For him such entities as electrons are themselves emergent from the yet more fundamental matrix of space-time. For him the ultimate terms are point-instants (pure motions). I cannot here discuss his fascinating but rather elusive treatment. As at present advised I can find no satisfactory foothold without electrons, or something of the sort, as *points d'appui.*

Be that as it may, there is clearly nothing in the foregoing thesis which necessarily precludes the further consideration of the same events from the point of view of Creative Evolution. The questions: What makes emergents emerge? What directs the whole course of emergent evolution?—these questions and their like are *there* quite in place. Furthermore, as between emergent thesis and Creative antithesis, Kant's 'Solution of the Third Antinomy' may afford a guiding clue.

If one selects, as above, certain salient phases of evolutionary progress, and lays stress upon them, one must remember that within the span of each phase there are other emergent sub-phases, some of them, no doubt, worthy of selective emphasis. Nay more, it must be realised that one is only attempting to classify the myriad instances of emergence in an ascending hierarchy. In all phases, in all sub-phases, and in all the myriad instances, there is continuity of advance, in that (*a*) there is never any unfilled gap or hiatus in the course of events, and in that (*b*) any instance, sub-phase, or phase, arises out of, is founded on, and implies, that which lies just below it in the scale.

Here, however, an important question arises. The selected sequence of qualities is—

(4) Conscious.
(3) Vital.
(2) Chemical.
(1) Physical.

Are the four terms of this sequential order homogeneous? If so, the quality of consciousness in (4) is homogeneous with the purely physical quality under (1). But this is not in accordance with a cardinal tenet very widely accepted—namely, that the physical and the mental cannot be regarded as homogeneous. They are, it is urged, essentially heterogeneous. On the assumption (which I feel bound to accept) that this traditional view is right, how does emergent evolution deal with the problem? It further assumes (or accepts as an hypothesis to be tried out on its merits) that there obtains a correlation of diverse (and in that sense heterogeneous) aspects. The word 'correlation' is here used to designate a mode of natural 'gotogetherness' which is *sui generis*; and the word 'aspects' (for lack of a better) to designate the fundamental difference between the mental or psychical and the non-mental or physical—a difference that must be accepted as something given in nature. On this hypothesis, then, how do our emergent phases now run?

Let me recall that each of our four emergent stages gives emphasis to a salient phase of emergence, and that within each phase there are sub-phases also emergent through the supervenience of something really new. Within the vital quality, for example, there are ascending sub-qualities. It is for the physiologist to deal with these. There are, too, in any given organism different lines of advance closely inter-related within the life-system of that organism as a whole. We must select, then, that line of advance which serves to enable us to interpret psychical advance in terms of correlation. Here we may be content, so far as the physiological aspect is concerned, to label, say, three sub-phases (*a*), (*b*), and (*c*); where (*c*) represents such integration as is

established in the cortex of the brain in correlation with reflective conduct; (*b*) such intermediate level of integration as is acquired in the course of individual life; and (*a*) such integration as is prior to (*b*) and (*c*) and on which they depend. I seek only to give a provisional schema. Now, I assume that correlated with (*a*) there is an affective form of psychical existence which is not yet consciousness as I shall presently define it; that correlated with (*b*) is consciousness of the order of such perceptual cognition as we impute to many animals; and that correlated with (*c*) is reflective consciousness or judgment which implies conceptual thought, and is often spoken of as self-consciousness. We may label these (thus provisionally distinguished) (α), (β), and (γ). They stand, I believe, in an order of dependence. We never find (γ) without (β) and (α); nor ever (β) without (α). The presence of reflective consciousness implies perceptive cognition; and the presence of perceptive cognition implies that of affective enjoyment. We do, however, seemingly find organisms with (β) and (α) but without (γ); and (as I think) lowly forms to which one can impute (α) without (β). Our tabular statement may therefore take some such provisional form as this, which may at least serve to indicate my method of approach.

(4) Vital	(*c*) .	(γ) Reflective judgment	Consciousness	(iv.)
	(*b*) .	(β) Perceptive cognition		
(3) Vital	(*a*) .	(α) Affective enjoyment	'The unconscious'	(iii.)
(2) Chemical		?	?	(ii.)
(1) Physical		?	?	(i.)

On the left-hand side of the table we have 'outer aspect'; on the right 'inner aspect.' The 'inner aspect' (if such there be) under (ii) and (i) is left with a query. Panpsychic speculation is here and now beyond our horizon.

What I may call the *homogeneous* precursor of the quality of consciousness (iv) is 'unconscious enjoyment' (iii) which, notwithstanding its negative prefix, must be regarded as a positive character. The *heterogeneous* precursor of (iv, γ) is (4 b). Heterogeneous treatment involves passing over from one 'aspect' to the other—e.g. interpreting perceptive consciousness in terms of brain-physiology, or psychical habit in terms of synaptic resistance. I do not mean to suggest that heterogeneous treatment is without value. Far from it. I do wish to suggest that we shall do well to realise that it *is* heterogeneous.

The Quality of Consciousness.

Before proceeding further certain preliminary questions must be briefly considered. First, is there progressively emergent evolution in consciousness? It is one of cardinal importance. My contention is that such evolution obtains in both aspects, inner and outer, the one in correlation with the other. This means that interpretation under emergent evolution is applicable to mental no less than to non-mental events. In other words, there is just as much progressive emergence in the inner or psychical aspect of organic nature as there is in the outer

or physiological aspect. This is the keynote of mental evolution throughout its whole range.

I regret here to depart from the conclusion to which Mr. Alexander has been led. Take such episodes in our mental life as seeing a rainbow, hearing a musical chord, partaking of woodcock, dipping one's hands into cool water. In Mr. Alexander's interpretation (as I understand it) percipient consciousness, in each case, differs only in what he speaks of as 'direction.' That alone is enjoyed. All further difference in one's cognitive experience on these several occasions is due to the difference in that non-mental set of events with which one is then and there compresent. Even feeling, as affective, is not itself enjoyed. Feelings are objective experiences of the order of organic 'sensa.' They are not in mind by way of attribute. We are conscious of pleasure and pain but are not differentially conscious *in* receiving them. Consciousness is here just compresent with certain phases of life-process. Thus, for Mr. Alexander, consciousness, alike in sensory acquaintance, in perceptive cognition, and even in feeling pleasure or the reverse, is itself undifferentiated (save in 'direction'); all the differentiation is in the non-mental world (beyond us or within our bodies) which is experienced and which transmits its characters to a recipient in which the rather featureless quality of consciousness has emerged. No doubt for Mr. Alexander the recipient is not merely passive; for there is mental process—not Agency, though he so often uses the word 'act.' But this mental process just actively takes what is given; and all the difference still lies in that which is given and not in the enjoyment of how it is taken.

But it is only when Mr. Alexander is interpreting consciousness at the perceptive level that he advocates this doctrine. When he deals with values or 'tertiary qualities,' such as beauty, his treatment is quite different. Consciousness hitherto featureless gives to certain objects of judgment their characteristic features. How, then, does the interpretation here run? 'In our ordinary experience of colour,' he says, 'the colour is separate from the mind, and completely independent of it. In our experience of the colour's beauty there is indissoluble union with the mind.' The contention comes to this. Colour resides in the thing seen, with which the organism having the quality of consciousness may or may not be compresent. Whether it is so compresent or not makes no difference to the non-mental existence of the colour as such. On the other hand, beauty resides, not in the thing only and independently, but in 'the whole situation,' which we may bracket thus [coloured thing *in relation to* compresent organism with quality of consciousness]. In that relation the object has a character which it would not have except for that relation.' The doctrine of 'internal relations' is accepted where beauty is concerned, and rejected in respect of colour. In other words, if the beautiful thing be one term and the conscious organism the other term, each gets its character (*qua* beautiful but not *qua* coloured) from its relation to the other. I should say that this holds good for the colour of the object, no less than for its beauty. My chief concern, however, is not with what Mr. Alexander rejects but with that which he accepts.

He holds (1) that the beauty of an object 'is a character superadded to it from its relation to the mind in virtue of which it satisfies, or pleases after a certain fashion, or æsthetically.' Now this being pleased or satisfied is referable (within the situation) to the organism which has the quality of consciousness, i.e. in brief to the mind. So far at least it seems to be a differentiated feature in consciousness no longer merely recipient. Mr. Alexander tells us (2) that, within the relational situation, 'the beauty is attributed to the object.' He says that 'it is the paradox of beauty that its expressiveness belongs to [I should say is referred to] the beautiful thing itself, and yet would not be there except for the mind.' He accepts (3) 'value' as that which satisfies a need; and he would (I think) not reject the view that it is primarily a felt need for behaving or acting (socially he would add) in some manner in regard to, or with reference to, the object to which value is attributed. He accepts also (4), as precursors of true values, what he calls 'instinctive values,' which I should speak of as the utilities of organic behaviour (e.g. under Darwinian treatment). We thus have (i) a specific mode of being conscious; (ii) reference of this differentiated feature in consciousness to the object; and (iii) a recognition of the pragmatic value of tertiary characters as determined by social conduct. I urge that, *mutatis mutandis*, the same treatment applies to the secondary characters; and that such treatment does away with Mr. Alexander's rather drastic difference of interpretation on the perceptive and on the reflective plane. In the case of secondary characters, no less than in that of values, we have (i) specific modes of being conscious, (ii) reference of this differentiating feature in consciousness to the object, (iii) as founded on the utility of behaviour thereto. Finally, we have Mr. Alexander's general conclusion. 'Thus value,' he says, 'in the form of the tertiary qualities emerges not with consciousness or mind as such, which the animals also possess, but with reflective consciousness or judgment.'

This conclusion seems to indicate that just as the quality of consciousness marks a phase of emergent evolution, with something genuinely new supervenient to the quality of life, so too within this phase there are ascending sub-phases of emergence. In reflective consciousness (the iv, γ of my table) there is, in 'value,' something genuinely new, supervenient on the perceptive consciousness (iv, β) which affords its evolutionary precursor. In other words, just as consciousness has its status in the hierarchy of salient qualities, so too within consciousness there are reflective and perceptive sub-qualities.

It is, I think, clear that the question I have here raised is of importance sufficient to justify the space I have devoted to it. It comes to this: Are there differentiating features ('*qualia*' they may be called) in consciousness as such? Do they, under conscious reference to objects, make these mental objects other than they would be if relation to consciousness were absent? If so, is this outcome of conscious reference restricted to the 'tertiary characters,' or is it also applicable to the 'secondary characters'? My belief is that it has to be reckoned with throughout the whole range of mental evolution.

The Place of Consciousness.

A second preliminary question is this: Where does consciousness dwell and have its being? From the point of view of emergent evolution I take it that the answer is: Consciousness is correlated with certain physiological and physical events which have place in the organism—and there only.

One has to deal with the relations which obtain in nature at their appropriate levels of emergence; and I hold that the proper level of spatial (and temporal) relations is that of physical events. But since all higher emergent strata depend on this stratum, and would not be present in its absence, space-time relations are implied throughout the whole series. In further illustration of what I mean, the proper level of energy-relations is sub-vital. Vital events, in a system which is not only physico-chemical but has also the quality of life, no doubt *depend on* energy-changes at the physico-chemical level. But there is no specific 'vital energy' (still less 'psychic energy') *in the same sense of the word 'energy.'* The word is then used with a different connotation.

Our present concern, however, is with 'the place of consciousness' under some meaning to be attached to this expression. There is so much ambiguity in the question: 'Where is it?' and in the answer: 'It is there,' that a little must be said thereon.

Suppose that one is dealing with things in one's room. Each thing is interpretable as a group of events with physical substratum. May we say, then, that any such given event (spatially related, of course, to other such events) has place in the group or system of events which is the thing? I take it that in one valid sense 'it is there.' In this sense a multitude of events—chemical, vital, unconscious, and conscious—have place which is dependent on that of the physical events in the organism. In this sense they are included in that system of events which we call the organism. But the organism is included in a larger spatial system. Shall we, for our present purpose, which is rather biological and psychological than physical, call this larger whole the situation? Now, suppose that my dog leaves the mat, on which he has been lying, to bask in a patch of sunshine near the window. He alters his place in the room as situation; but the physical, chemical, and other events retain their place in him. Whither he goes, thither also go all these events. In one sense they are still there—wherever he may be. In another sense their place has changed. They were, a few minutes ago, on the mat; now, they are near the window.

And if we ask: Where does the dog behave? I take it that the natural answer is: In the environing situation. But the question may be taken to mean: Where do his muscles function? Then the natural answer would be: In his body wherever it may go under their functional action. Let us next ask: Where does the dog perceive? In one sense he perceives in the situation, which includes the thing seen and the dog that sees it. But in another sense the perception is in him. That is where the process of perceiving (or more strictly the physical events correlated therewith) may be said to occur and to have place.

We should distinguish, then, a 'there of place' within the inclusive situation, and a 'there of place' within the thing included in that situation. But when the dog rose from the mat and walked to the window, the influence of the sunlit patch which took effect on his eyes was the basis (founded on prior behaviour) of reference to place near the window. That is where he saw it; it is towards that spot that his procedure was directed. Now, on this occasion, reference to place within the situation, and behaviour in reaching that place, were happily consonant. There was nothing of the nature of illusion. But when one of his predecessors in my household barked at his mirror-image, the place of reference for behaviour was not consonant with what sophisticated human folk call the 'real place' of that thing towards which he behaved. The conditions were abnormal; and 'place of reference' did not coincide with 'real place.' So, too, if you see a pike below the surface in a still pool, and try to shoot him with a saloon rifle, you will probably miss him (unless you have learnt the trick) because the place of reference for behaviour in aiming is not the place where he happens to be. Coincidence of place of reference with 'real,' or acknowledged, place can only be established through the outcome of behaviour, crude at first, intellectually guided at last. If this behaviour *works*, well and good in the realm of practice; but it may work admirably, and yet not stand the test of validity in the realm of interpretation which includes the problem of cognition. In any case we have to distinguish (*c*) the 'there of reference' from the 'there of place' (*a*) in the situation and (*b*) in the thing. The 'where' to which the colour of the ruby, and to which its beauty also, *is referred*, is unquestionably 'there' in the gem; the conditions for colour-perception are undoubtedly 'there' in the cognitive situation as a whole; but the chemical changes due to electro-magnetic influence are 'there' in the retino-cerebral system of the organism. And it is in correlation with these changes which run their course within the organism that the quality of consciousness is emergent.

Does it follow from what has been said that 'the place of consciousness' is in some differentiated part of the organism—say in the cortex of the brain? Again the question is ambiguous. In what sense has it place there? Certain focal events in what one may call the intra-organic situation have place there; and in that sense conscious enjoyment is the correlate of physiological changes that may there be localised. It is dependent on these changes, and in their absence would not be present or in being. But it is no less dependent on all that takes place in the intra-organic situation. Hence in the wider sense nothing less than the whole organism as a going concern is the seat of the enjoyment which is correlated with its total working as a vitally integrated system.

Consciousness as Objective.

It will now, I hope, be sufficiently clear that when I say that consciousness dwells and has its being in the organism, and there only, my meaning is that any given instance of the class of events we call conscious (in a comprehensive sense of the word) is correlated with certain vital and physical events which have place in that organism.

There is, however, a very different but still quite empirical view. It may be said that consciousness as such embraces all the objects to which there is conscious reference. In other words, on this view it pervades the whole situation. That is 'where it is.' Let us consider what is here meant. It rests on the interpretation of consciousness as a mode of connection under which objects in the whole situation are the terms related *inter se*. Hence Professor Woodbridge urges that objects are 'in consciousness' in the same sense as things are 'in space' or events are 'in time.' Just as the expression 'in space' or 'in time' conveniently condenses the longer and more cumbrous expression 'in that kind of inter-relatedness of things which is called spatial or temporal'; so too does 'in consciousness' condense the fuller expression 'in that kind of inter-relatedness of objects which we call conscious.' And just as there is a spatio-temporal continuum within which things have place; so too, according to Mr. Woodbridge, 'consciousness may be defined as a kind of continuum of objects.' We should, therefore, he says, deal with the relations of the objects in consciousness to one another 'in the same way as that in which we deal with the relations of things in space to one another.' It is, I think, clear that consciousness, on this view, is coextensive with what is sometimes called 'the field of consciousness'—that of which one is conscious in reference thereto. In other words, consciousness is nowise limited to the organism, but is a special kind of relatedness which pervades the whole conscious situation. In my phraseology following Berkeley, the field of consciousness is 'in mind by way of idea' but not 'in consciousness by way of attribute.' But we are here considering a different usage under a different terminology.

Professor Holt, whose avenue of approach, like that of Mr. Woodbridge, is primarily logical, and new-realistic, develops an interesting doctrine of 'neutral entities.' I cannot here parenthetically discuss this doctrine with its stress on the objective reality of universals independently of consciousness. We may, I think, for our present purpose, take his view to be that what we commonly call the environment of an organism is *au fond* constituted by those universalised neutral entities we name objects, and that it is these neutral objects which call forth in the organism its specific responses or its more highly organised behaviour. But not all the environment calls forth such response or behaviour. That part which does so on any given occasion is what Mr. Holt calls a 'cross-section.' It is, so to speak, the business part of the total environment—that part which counts for behaviour—and it is through behaviour that it is selected from the rest of the environment. Now this neutral cross-section, defined by responsive behaviour, 'coincides exactly with the list of objects of which we say that we are conscious.' Mr. Holt therefore, on the basis of this coincidence, feels himself free to call the environmental cross-section the 'psychic cross-section,' or 'consciousness' or 'mind,' within which the individual members are 'sensations,' 'perceptions,' 'ideas,' &c. It is clear, therefore, that for Mr. Holt as for Mr. Woodbridge, consciousness is, or includes, all that part of the environment to which there is conscious reference; that it 'is extended both in space and time . . . being

actually such parts of the object as are perceived'; and that the cross-section we are said to perceive is coincident with that towards which we behave.

Obviously we have in Mr. Holt's doctrine one form of a behaviouristic interpretation of consciousness. This opens up an issue which cannot here be discussed. One can, however, briefly indicate what seems to be the essential question at issue. Let us provisionally grant that organic behaviour towards what we call an object is 'coincident' with conscious reference to that object; nay more, let us grant that in the absence of prior behaviour to it there would never be evolved such conscious reference to it. Even granting this, does it follow that this conscious reference is not only 'coincident with' behaviour but is nothing more than that behaviour? In accordance with the principles of emergent evolution it does not follow, and it is not so; the one is a function of the organism's life, dependent on, but emergently more than, its physico-chemical constitution, while the other is a function of that organism's consciousness, dependent on, but emergently more than, that organism's life. And just as life is a quality of the organism that behaves and is centred therein; so too is consciousness a quality (higher in order of emergence) of the organism which is conscious in perceiving and in behaving.

Modes of being Conscious.

We may revert, then, to the view that the quality of consciousness has place in the organism (or more strictly is correlated with physical and physiological events which have place there), but that conscious reference, like organic behaviour on the plane of life, is effluent from that organism which receives physical influence.

My position under genetic treatment is this: (*a*) Physical processes of many and varied kinds are, on occasion, influent on the organism which has receptors attuned to them; (*b*) very complex changes, physico-chemical and physiological, are called forth in that organism; and (*c*) it responds in organic behaviour, coming thus into new fields of physical influence. Thus, under light-radiation, influence from that which, in the language of our highly developed adult reference, we call a ladybird, affects the retinal receptors of the chick a few hours old; organised changes in his tissues result therefrom; he pecks at the ladybird, and tango- or chemo-receptors are thus physically influenced. So far the interpretation is biological and behaviouristic only. But, rightly or wrongly, I impute to the chick affective enjoyment on which some measure of conscious cognition is founded. *That* is neither physico-chemical nor physiological, though it is correlated with both. It is mental. It is the psychical or inner aspect of processes which have also their outer aspect with which the bio-chemist and the physiologist deal. But it just as truly belongs to that organism as does its life, or its chemical constitution.

We want now to come to closer quarters with this inner or psychical aspect correlated with the whole range of life-processes (*a*), (*b*), and (*c*). What is it? Perhaps all that one can say in reply to this question is that what it feels like that it is. But one can enumerate different ways

in which we enjoy this inner aspect—in which we are conscious in the most comprehensive sense of the word. Thus when one is seeing, hearing, tasting; when one is running, climbing, swimming; when one is imaging in reverie or in dream; when one is irritable, worried or anxious; joyous or sad, in discomfort or at ease, fit and well, sick or sorry; when one is thinking or trying to recollect, following an argument, or solving a problem; accepting some statement in an attitude of belief, rejecting it, or poised in a state of doubt; when one chuckles over a joke, or winces under a bad pun; when one vibrates to music or shudders at the braying of a street band; nay even when, thereafter, 'silence like a poultice comes to heal the blows of sound'; in all these cases, and in a thousand others, we have instances of what it is to be conscious in the most comprehensive sense.

It will of course quite rightly and pertinently be asked: Who or what is thus conscious, now in this way and now in another? The empirical reply to this question (that to be given under emergent evolution) is: The integrated system of all the fluent conscious events that *are* thus integrated within that system. That is just what the mind is—an integrated system of consciously inter-related terms intrinsic to the organism and correlated with its life. No doubt a further question lies behind: What is it that gives to such a system the integration that it has? It is here that Creative Evolution offers an explanation in terms of Agency. In accepting the 'given' as that which we find in nature—and in leaving the question: What gives? to be discussed in the philosophical class-room—emergent evolution does but follow, as I think, the traditional procedure of science.

Consciousness and Enjoyment.

In speaking of a mind as an integrated system of conscious events the word 'conscious' is used in the broad and comprehensive sense that was almost universally accepted a generation ago. But in accordance with current usage we must now distinguish consciousness from the unconscious. I happen to regard the word 'unconscious' as peculiarly unfortunate—chosen as it is on the *lucus a non lucendo* principle. But let that pass. There it is and we must make the best of it—seeking to penetrate its dark wood. Under the older and more comprehensive use, consciousness may be indefinable. As in the case of spatial or of temporal relatedness we have got down to something that we find, rather than to something that can be strictly defined. Hence one has to proceed by indicating instances that fall within the inclusive class which we so name. The position is that, in the comprehensive class which we used to comprise under the heading of consciousness, it is now thought desirable to make two sub-classes—(*a*) the unconscious and (*b*) the conscious. There is call, therefore, for the indication of some criteria which shall serve to distinguish the one from the other. Here definition is required. And since the unconscious is 'served with the negative prefix,' it is clear that the criteria we seek must distinguish by their presence the conscious from the unconscious in which these criteria are absent. Under what heading, then, are we now to place the comprehensive class including both (*a*) and (*b*)? I suppose we

may call it the class of psychical events—as distinguished from physical and physiological events. But we still want some convenient noun which we may qualify by the adjectives 'conscious' and 'unconscious.' I borrow from Mr. Alexander, and adapt for my present purpose, the name 'enjoyment.' Perhaps the chief objection to the choice of this word is that it must be understood as including what is unpleasant no less than that which is pleasureable. But as I cannot find a better, and am loth to coin a worse, I ask leave to use this word 'enjoyment' to include all that has the psychical character or aspect. I regard the emphasis on affective tone which it suggests as a point in its favour.

On these terms there fall within the comprehensive class of enjoyment two sub-classes: (*a*) unconscious enjoyment and (*b*) conscious enjoyment—the latter marked by certain differentiating criteria. It may, however, be said (with some impatience): This division and subdivision into classes and sub-classes may be all very well in its way; but we ought to deal with concrete systems, not with abstract classes. So be it. Then, in this or that psychical system or mind, with concrete individuality, there is enjoyment which is (in some sense) unconscious, and there may also be enjoyment which is conscious (under some definition); and we want to distinguish in some way the one kind of enjoyment from the other. That puts the matter in more concrete form.

The question now arises: Is the distinction between the conscious and the unconscious just the same as that which is often drawn between 'above the threshold' and 'below the threshold' (supraliminal and subliminal)? Or, if they are not just the same, is there such close and intimate alliance that we may still say that all that is supraliminal is conscious and all that is subliminal is unconscious?

Let us first ask what we are to understand by supraliminal and by subliminal. I find this question exceedingly difficult to answer, save in rather vague and general terms. It involves a boundary line—the threshold—very hard to draw if one keeps within the sphere of what I have called homogeneous treatment. Is it a matter of intensity of psychical process, or of complexity, or of some combination of both? If so, can we, on purely psychical grounds, get a scale of one or other or both, so as to determine that zero-level at which what we call the threshold stands? It is difficult to do so.

May we say that the supraliminal is what we actually feel or experience, and that the subliminal is that the presence of which we infer? Then on what grounds is this inference based? Is it that we find, on occasion, that we have done something without any felt experience in doing it? If so what evidence is there with regard to the nature of the psychical or inner aspect which *on that occasion* accompanied the doing? Or is it that the supraliminal experience is such as to lead us to infer that the subliminal modifies its felt nature? But if the difference is felt, as such, the subliminal so far enters into the supraliminal field so as to be felt indirectly if not directly. In that case the boundary seems hard to draw.

Shall we then resort to heterogeneous treatment? Shall we regard the psychically supraliminal as correlated with some assignable

character of physiological process, say in the cortex of the brain? It is sometimes said that 'when the brain-paths are worn smooth' the correlated psychical process becomes (or tends to become) subliminal. Without denying the partial validity of some such interpretation in correlating the physiology and psychology of habit, can one accept the general principle that at some stage of lessened synaptic resistance enjoyment is subliminal and at some stage of heightened synaptic resistance it is supraliminal? Is there not rich enjoyment (apparently well above the threshold) in the performance of well-established habit? And is there good evidence that (let us say) clear and vivid perception or swift and effective thought (which seems to thrill with supraliminal enjoyment) is proportional to physiological friction or synaptic resistance?

If the emphasis fall, not on the synaptic resistance overcome but on the establishment of a constellation of neuronic connections, and if it be urged that it is integration in progress which is correlated with what is psychically supraliminal, there may be much that is in favour of some such view. But does it follow that it is only when integration is in progress that enjoyment is supraliminal? There is surely much which is the outcome of well-established process that seems to be distinctly above the threshold. I am not satisfied that in our present state of knowledge heterogeneous treatment helps us very much to draw a definite line.

Reverting, then, to homogeneous treatment, it is often said that the subliminal (commonly regarded, I think, as a synonym for the unconscious) may best be defined as that which lies beyond the reach of introspection. But the introspection in terms of which the distinction is made stands in need of careful re-examination. Apart from behaviourist criticism, which has to be reckoned with, Mr. Alexander has raised the pertinent question: What is it that is reached by introspection? Is it the process of minding (e.g. attending or being interested), or is it that which is minded (what one is attending to, or interested in)? If the latter, he denies that it should be called introspection; it is a form of 'extrospection' in relation to what, for him, are non-mental (and, for me, objective) images or concepts—in mind by way of idea. And if the former, he denies that there *is* such introspection; for minding, though it is enjoyed, cannot, he says, be an object of contemplation or at the same time then and there minded. Now I have taken the word 'consciousness' as connoting mental process—i.e. that which is in mind by way of attribute. And I am disposed to agree with Mr. Alexander that mental process as such (and therefore consciousness as such) is not directly within the reach of introspection. I cannot follow this up. Indeed, my aim is only to show that if we are to define the supraliminal in terms of introspection we need a careful and up-to-date discussion of that in terms of which we so define it. To say that everyone knows what introspection is does not suffice.

Furthermore, those who have carefully considered the matter will probably regard introspection as possible only at the level of reflective thought. Presumably the cow has not reached that level. But if the

supraliminal is to be defined as that which is within the reach of introspection can the cow have any supraliminal enjoyment if she have no introspection by means of which to reach it? Does comparative psychology endorse this current method of dealing with that very elusive limit, the threshold?

It must not be inferred from what I have said that the concept of threshold must be abandoned. It may be a difficult line to draw and yet be there as a boundary. We may still speak rather vaguely of supraliminal and subliminal. What I wish to suggest is that the line between them need not be coincident with that between conscious and unconscious. There are, I believe, modes of enjoyment both conscious and unconscious in the supraliminal field. But this reopens the main question: What are the differentiating criteria of the conscious?

Criteria of Consciousness.

Ask the plain man what he means when he speaks of acting consciously and he will probably reply: 'I mean doing this or that with some measure of intention and with some measure of attention to what is done or to its outcome. The emphasis may vary; but one, or other, or both, of these characterise action that I call conscious. If I offend a man unconsciously there is no intention to give offence. When a cyclist guides his machine unconsciously he no longer pays attention to the business of steering, avoiding stones in the road, and so forth.' Now if this correctly represents the plain man's veiw, it is clear that a full consideration of his attitude would involve careful discussion of intention and of attention. This is beyond my present scope. I want to dig farther down so as to get at what, as I think, underlies his meaning, and thus to put what I have to submit in a much more general form.

I want, if possible, to get down to what there is in the most primitive instances of consciousness—i.e. right down to that which characterises them as such. I believe that there is always in addition to that which is immediately given (say under direct stimulation in sense-awareness) some measure of revival with expectancy, begotten of previous behaviour in a substantially similar situation. Consciousness is always a matter of the subsequent occasion, and always presupposes a precedent occasion. In other words it is the outcome of repetition; and yet, paradoxically, when it comes it is something genuinely new. But this is the very hall-mark of emergence. That is why Mr. Alexander and I speak of consciousness as an emergent quality.

Let us analyse some simple first occasion—that on which a chick behaves to a ladybird will serve. The eye is stimulated from a distance with accompanying enjoyment (*a*). The chick responds by approaching and pecking with enjoyment in behaving (*b*). There follows contact stimulation with its enjoyment (*c*); and, thereon, behaviour of rejection with (*d*). We have thus (as I interpret) a biologically determined but orderly sequence affording successive modes of enjoyment *a*, *b*, *c*, *d*. So far the precedent occasion. On a subsequent occasion there is (*a*) as before in presentative form; this is immediately given in sensory acquaintance. But (*b*, *c*, *d*) are also 'in mind'—

mediately or in re-presentative guise, under revival, as what Professor Stout calls 'meaning.' We have therefore (under an analogy) on the precedent occasion the notes *a*, *b*, *c*, *d*, struck in sequence. We have on the subsequent occasion (*b*, *c*, *d*) rung up by (*a*) through a 'mechanism' (a bad word since the mechanical is superseded) provided psychically and neurally in the instrument. And when the notes (*a*, *b*, *c*, *d*) thus vibrate together they have the emergent quality of what one may speak of as the *chord* of consciousness.

What is there, however, about this emergent chord which differentiates it from the precedent sequence of notes *a*, *b*, *c*, *d*? It must be something psychical in its nature. I suggest that the revival carries with it a specific mode of new enjoyment which may be called 'againness'; that which affords the basis of felt recognition. There is also something equally new in expectancy. That this is (so far as our own experience testifies) a factor in the chord of consciousness is, I should suppose, scarcely open to question. I believe that it arises somewhat thus. On the precedent occasion the order of sequence was (*c*), *after* (*a*). On the subsequent occasion the *quale* in consciousness takes the form of what one may call the 'comingness' of (*c*) precedent to the 'comeness' which normally follows. But I cannot here follow up this clue.

Now whereas on the precedent occasion it is behaviour unconsciously directed towards that from which stimulation arrives that determines the order *b*, *c*, *d* as sequent on *a*; on the subsequent occasion it is the 'meaning' (*b*, *c*, *d*) which then consciously determines the direction of behaviour. This centering of 'meaning' on that to which behaviour was on the precedent occasion unconsciously directed is the basis of conscious reference to an object.

The characteristics, then, of a chord of consciousness are revival with expectancy and with conscious reference which anticipates, and, through anticipation (thus forestalling the event), may endorse or inhibit, the further course of behaviour. And its emergent character, as chord, makes consciousness, not merely an additive blend of constituent tones of enjoyment, but (in Browning's forcible emphasis on a wholly new quality) 'a star.' (Cf. *Abt Vogler*.)

I have thus far dealt with the criteria of consciousness on the lines of what I conceive to be its evolutionary genesis. I must now ask whether these criteria—revival with expectancy and reference—do not characterise what we commonly regard as conscious enjoyment in our own adult life. My own experience is consonant with the outcome of genetic treatment. And I would ask others if there is not in our current consciousness always some measure of felt 'againness' carried over from the past in revival, and always some measure of 'comingness' in expectancy. I would ask whether there is not, as essential to consciousness, some leaning back on previous experience, some leaning forward to that which the future has in store. Is not this what M. Bergson means (I do not say all that he means) when he speaks of consciousness as 'a hyphen' linking past and future?

It need only be added that the conscious enjoyment in minding lies in the felt againness and comingness and referring—i.e. in the *ing*

aspect. But there are in consciousness as above differentiated always correlative *-ed*-aspects in that which is revived, that which is expected, and that to which there is objective reference.

Stress on Integration.

I suppose that there may be pretty general agreement that, in dealing with mind, emphasis—perhaps the chief emphasis—should fall on integration. I use the word ' integration ' for that kind of systematic relatedness which obtains in an organism, and in a mind, where the functioning of sub-systems, as parts of the whole, depends on that of the system as a whole.

Let us here very briefly advert to that organic integration which characterises a system which has the emergent quality of life. We find a number of sub-systems—respiratory, circulatory, reproductive, and so on—within the comprehensive life-system of the organism. We find these functional activities interrelated in many very subtle and delicate ways in the life that is common to them all. We consider, for example, the integrative action of the nervous system, and of that which may now be called the ' hormonic ' system of internal secretions distributed by the blood-stream. The working of any one sub-system may facilitate or enhance the working of another; or it may partially arrest or even inhibit it. Abnormal functional activity of one sub-system may throw another sub-system out of gear; and so the trouble may spread. But the sub-systems are not historically prior to the life-system as a whole within which they play their parts; nor is the whole (*that* whole) prior to its sub-systematic constituents; whole and parts have been progressively evolved together with such closely related interplay as characterises the quality of life.

Now I assume, or accept as a provisional hypothesis, that unconscious enjoyment is correlated with life-process throughout its whole range. I know not where to draw the line between presence and absence. And how else can we interpret in homogeneous treatment, under emergence, psychical continuity in the race? In other words, wherever there is life there too, even in the germ cells, there is also, I assume, an accompaniment of enjoyment, psychical in its nature, at a level correlative with that of the current physiological process.

If this hypothesis be provisionally accepted in the spirit in which it is provisionally offered, what holds good for the life-system holds good also in principle and *mutatis mutandis* for the psychical system. But within that system there emerges the higher quality of consciousness (iv, β) characterised, let us say, by cognitive reference to the objective environment (to emphasise this criterion). Hence in the light of developing consciousness there is a progressive re-grouping in reference to the objects of which we are conscious, or objects in terms of which much of our unconscious enjoyment is re-interpreted. We say that dispositions, or interests, or innate tendencies, or emotional systems, or instincts, or impulses, are awakened to activity from a state of more or less unconscious slumber. (We are sure to use some rather metaphorical expressions.) These are then regarded as the sub-systems of the mind. Each has some measure of autonomous integration; all

are in some measure inter-related; and in a well-balanced mind, the net results of a bewildering number of psychical processes, many of them previously subliminal and unconscious, are caught up in subservience to conscious integration. But taken in detail there is much interplay between the psychical sub-systems as such, with facilitation, partial arrest, more or less inhibition, and perhaps derangement of function. There may be failure of normal integration within one systematic whole, or even such dislocation as we speak of as complete dissociation. And any of the psychical sub-systems—the so-called sexual complex for example—may be active in the subliminal region of the unconscious, or may rise into the supraliminal field and may modify the course of conscious events.

There is thus integration within the sub-systems severally, and integration of these sub-systems collectively so as to constitute a whole with (let us hope) due balance and poise. The unity of the whole is not that of simplicity but that of integrated complexity. In the degree in which the total integration fails to conduce to what we speak of as mental health and sanity we regard the poise as abnormal, and seek, by appropriate means (under the guidance of sympathy), (1) to ascertain to what sub-systematic conditions the lack of balance is due, and (2) to re-establish, if possible, the normal poise. It is here that psycho-therapy has done such valuable work in the practical application of psychological principles no longer restricted to the sphere of reflective consciousness only.

Levels of Psychical Integration.

In our normal life much integration proceeds on the reflective level—that of rational thought and volitional conduct. The older philosophers, with some variation of terminology, urged that the difference between this reflective level and the perceptive level below it (e.g. in Descartes' animal automatism) is one not only of degree but of kind. The difference, they said in effect, is radical and absolute, demanding metempirical explanation. Thus the word 'kind' carried a definitely metaphysical implication the influence of which is still with us to-day. But apart from this, as a matter of frankly empirical description of what is found, it was their way of expressing what I seek to express by saying that reflective consciousness has a new emergent quality—that which characterises reason as distinguished from perceptual intelligence. We have, however, the one word 'consciousness' for both these levels. But within the more comprehensive sub-class, comprising all instances of consciousness, we may distinguish two sub-classes subordinate therein, (i) that of instances of reflective consciousness (iv, γ), and (ii) that of instances of non-reflective consciousness (iv, β). Both sets of instances have the criteria of consciousness. But in (i) there is a further *differentia* in that 'value' (in the technical sense) is referred to the object of such reflective thought. There is then, on this view, reflective integration, and there is also non-reflective or perceptive integration, each on its appropriate level, and each in its distinctive way conscious.

It is to the reflective level that all interpretation and explanation

properly belong. And it is here that there emerge the significant relations of conduct to value (truth, beauty, goodness) in conscious reference to objects of reflection. That, in us, much integration is established at this level of our conscious life cannot be questioned. But to say that all psychical integration is established at this level is itself an interpretation subject to truth-value; and one is pretty safe in roundly asserting that it is erroneous. Now, regarded from the point of view of emergent evolution, just as the quality of consciousness is dependent on, and supervenient to, the quality of life, so too is reflective consciousness dependent on, and supervenient to, the prior development of unreflective consciousness—in human folk in large measure begotten, through perceptive imitation, of the customs of our 'herd.' This unreflective process, as such, imitatively follows a lead which is itself the outcome of traditional habit no less unreflective. But reflective interpretation in due course supervenes when values come within the mental horizon; and it may be (alas often is) erroneous. And a leading type of false interpretation to which men are prone is seen in the tendency to trump up reflective motives in terms of value for actions determined by integration that is unreflective in character. As Huxley long ago put it, 'What we call rational grounds for our beliefs are often extremely irrational attempts to justify our instincts.'

How then do we stand? There is perceptive integration (consciously but not reflectively established) such as is the salient feature in the mental life of many animals. This passes up from its proper level to that of reflective consciousness, and is there re-integrated in the new significant field of value. Then, as reflective habitudes of valuing get firmly rooted, such re-integration spreads downwards to give value to more and more of that which has been established under the lower and earlier integration of the perceptive order. Behaviour is reorganised as conduct in terms of value.

This double process is noteworthy. When the emergent level of reflective consciousness is reached, the outcome of prior unreflective integration passes up from its lower level. But as re-integration at the upper level proceeds, more and more of the unreflective substratum undergoes reflective regrouping around the values which are the new centres of that higher re-integration. Unreflective integration ascends from below; reflective re-integration descends from above. But they are different; the new 'form' of integration is other than the old. There is always some 'conflict' which has been a fruitful theme in drama from the time of the Greeks onwards. And in our so-called normal life (to say nothing of that which is abnormal) this conflict of systems with different centres of grouping and fields of influence, is daily and hourly in evidence.

Now carry the matter a stage lower. Unreflective integration of the perceptually intelligent order is consciously established in the course of individual life. The animal unreflectively learns to act in nice accordance with varying circumstances just as man learns also to act reflectively in relation to value. But is there not a yet deeper integration the products of which come up from below the perceptive level? Unquestionably there is. A generation ago it was regarded as purely

physiological; but that involves heterogeneous interpretation. If we accept the correlation of enjoyment with life we can regard it as unconscious integration. Its characteristic feature is that it is not consciously established in the course of individual or personal life. Its integrated 'form' is inherited and not acquired, though it may be swiftly re-integrated at the perceptive or (later) at the reflective level. As such and in its primary 'form,' as initially given, it is an ancestral bequest transmitted as a psychical legacy through the parents. Of it the individual is the unconscious heir.

Primarily dependent on the great life-functions, closely correlated with current physiological process in the organic sub-systems, finding expression in the co-ordinated, albeit unlearnt, behaviour adapted to racially recurrent life-situations, unconscious enjoyment, as psychical aspect, is, as M. Bergson says (but under a different interpretation), moulded to the very form of life—nay more, to every changing phase of the physiological balance and poise of the organism as such. Of this unconscious enjoyment much is, and may remain, the subliminal basis for a supraliminal superstructure at the levels of conscious integration. But *qua* unconscious it does not necessarily remain subliminal. In any of its 'forms' it may, on occasion, surge up into the supraliminal field with strongly affective tone, and thus afford new factors to be woven into the tapestry of the higher conscious integration. It still, however, even there, bears the mark of the unconscious in that it is new and unexpected with no feeling of againness just because, as fresh and new, there is no againness in the individual life to feel. And this insurgent factor, welling up from the unconscious, may, and often does, come into conflict with the outcome of perceptive or unreflective, and still more markedly with the outcome of our reflective re-integration. This more radical conflict is *au fond* that between what is racially established for the furtherance of life, as such, and what is socially established (far later in the evolutionary order) at the reflective level of that which we call (with emphasis on one of the values) our morality.

Having no space for further elaboration in detail, I must rest content with drawing attention to the following salient points. Although it originates in the unconscious and is there shaped to its integrated 'form,' the uprush from the deeper psychical strata founded on life-inheritance may glow and thrill with the affective tone which is the hall-mark of enjoyment and may take a very high place in the supraliminal field. Secondly, in that field it can only be distinguished (and that mainly inferentially) under analysis. It cannot ever be separated from the conscious factors which emergently combine with it in perceptive or in reflective re-integration. Thirdly, the distinguishing positive character of the innate factor which comes up from the unconscious (if we can catch it prior to further combination) is that it is new to individual experience. As new, there is no revival, no feeling of againness, no expectancy of what will next come based on the experience of what has come on like occasions; for there have been no like occasions in the course of individual life. And it gets all its reference to objects through its alliance with the conscious.

It seems to me therefore imperative to distinguish in that which is present in the supraliminal field according to the mode of origin of the integration that obtains. We must ask: How far is the 'form' which it assumes (iii) the outcome of reflective integration; (ii) the outcome of unreflective or perceptive integration; and (i) the outcome of the integration in the subliminal unconscious to which as living beings we are heirs? If I am right in regarding (ii) and (iii) as successively emergent qualities of consciousness there is somewhat of a leap (though no breach of continuity) from (i) to (ii), and from (ii) to (iii). There is always something more (involving new terms in new relations) in the higher-level conclusion than is contained in the lower-level premisses. This is the cardinal principle of all emergent evolution. Without this there would be nothing really new—merely a reshuffling of the old.

Revert now to ascending and descending integration. Under what may be spoken of as degradation—going down a step with habitude and habit—well-established reflective integration may assume the status of unreflective integration, and well-established unreflective integration that of the unconscious. The illustrative facts are familiar enough. It appears that the physiological correlates of this descent or degradation from higher to lower levels may be interpreted in terms of neural loop-lines and lower-level short-cuts due to lessened synaptic resistance in subordinate centres. If this be so, it is strictly accordant with the dependence of consciousness on life that psychical degradation should accompany physiological automatisation. The one is the correlated inner aspect of processes with which the physiologist has to deal.

Are there Unconscious Images and Ideas?

In the interpretation to which I have been led unconscious enjoyment (not necessarily involving unconscious images and ideas) is no less integrated than is the system of physiological events which gives to life its emergent quality. If the analogy be permitted, just as in the physiological symphony of life there are chords and phrases and motifs, each with an emergent character of its own (e.g. the part played by the instruments of the reproductive sub-system), so too, in the psychical symphony of unconscious enjoyment there are correlated chords, phrases, and motifs. And all goes well so long as due balance and harmony is maintained in the orchestral performance, no matter what instruments play a dominant part at the time being. But unconscious enjoyment is primarily inherited psychical music correlated with the outcome of life-inheritance. I entertain little doubt that the life of animals, could we only feel its inner aspect as they themselves do, is brim-full of a rich music of unconscious enjoyment. As I write the swifts are wheeling and shrilling in the summer air. Am I wholly wrong in imputing to them an integrated form of enjoyment which is theirs on a basis of inheritance? Perhaps even sympathetic naturalists fail adequately to realise to what extent in animals the business of life as such, with further life as its wage, has also its psychical reward in enjoying so fully the performance of life's job. And this reward in the enjoyment of doing is inherited with the ability to do. A

behaviourist interpretation of how it all comes about is, I believe, perfectly sound in its way. Not in what it emphasises, but in what (among extremists) it ignores—a psychical factor—does it seem to me to be deficient. In us at any rate the presence of enjoyment is undeniable. And though it is so readily caught up into consciousness it still carries, I think, the marks of its unconscious origin. What does the poet or the artist tell us? Does he not claim that what springs up within him—if it be in truth (he may add) in any valid sense *his*—is quite inexplicable on what he regards as psychological principles? And if psychological principles deal only with conscious integration he is right. His poetry, or his art, is not in its essential nature the outcome of perceptive or reflective integration. Its well-springs lie deeper than that in the unconscious. He rightly affirms that the real thing in all true art is beyond his conscious control, though the means by which it is expressed must be learnt and may be bettered by taking thought. This is enshrined in the proverb: *Poeta nascitur non fit.* And even of those who can only appreciate his work, may it not be said, with a touch of paradox, that enjoyment in art becomes reflectively conscious in criticism. This need not mean that the critic enjoys poetry any the less for the combination in higher integration of unconscious and conscious enjoyment. What it does mean is that the glad newness and glory of surprise lies in the poetry and not in the criticism. Once again it must be said that it is the fresh unexpectedness that is still the hall-mark of the unconscious.

And here a question arises which I find it difficult to put in readily intelligible form. Is the rich enjoyment which gets human expression in the poet—but gets expression also in the Black-cap, consummate master of song—is this enjoyment dependent on that expression, or is the expression dependent on unconsciously integrated enjoyment? Which is prior to the other in order of dependence? What, you may ask, am I driving at in propounding so subtle a conundrum? Well, I take it that the Black-cap sings, under the conspiring influence of the situation and environing conditions, because it is part of his inborn nature so to sing under these circumstances. His song is primarily the outcome of the unconscious poise of a psychical system, correlated no doubt with a physiological poise. In that sense surely the expression in song depends on unconscious enjoyment—or, if it be preferred, the behaviour in song depends on the integrated life-process with which unconscious enjoyment is correlated. Whether we say that the behaviour-expression (with *its* accompanying enjoyment) is dependent on impulse, or disposition, or instinct, or emotional state, what we mean is that if the latter be absent the former will not come into being. If I may so put it, unconscious enjoyment, affectively integrated, becomes clothed in the expression, with *its* enjoyment, and is consciously integrated therewith on the higher perceptive level. And what of the poet? I think that he too may tell us that unconscious integration of the emotional order precedes the imagery in which it is expressed—that, as he may put it, 'the poetic inspiration strives to find expression'—that the clothing in imagery depends on the prior affective integration, as yet unconscious.

This leads on to the broader question. Does that which we call the unconscious depend on the presence of images and ideas; or are images and ideas the cognitive raiment which the unconscious puts on at the emergent levels of perceptive and reflective consciousness? The question in brief really comes to this: Are there what we may comprehensively speak of as memories in the unconscious? In much present-day resuscitation of Herbartian notions (which some of us thought were little better than picturesque mythology long ago discarded as obsolete) the unconscious is peopled with such memories—with images, ideas, wishes and thoughts, living together, as Professor James Ward puts it, 'like shades on the banks of the Styx.' Is this so? It is against this sort of thing that the behaviourist rises in vigorous protest; and, swinging his pendulum too far (in some cases), drops psychology overboard and proceeds on his course in the biological ship. For those who cannot go to this extreme the alternative view is that memories have being only in supraliminal consciousness and that the unconscious, as such, is no wise imaginal. It is not yet cognitive. Only through cognition at the higher level of unreflective or perceptive consciousness does it begin to put on the raiment of images, ideas and the rest, and thus find expression in the supraliminal field.

On this alternative view not only are there no inherited memories in any form or guise, but there are no memory-images in existence save as correlative to an existent process of conscious remembering. One opens up here the whole problem of retention. What is retained—the blossoms of imagery, or the conditions under which they will in due season appear? The plant does not retain flowers; but its abiding nature is such that flowers are put forth under the influence of external conditions at a recurring stage of constitutional life-balance. This analogy may be rejected. If so the grounds of rejection should be clearly set forth. Is it on the ground that lilac-blossoms are *not* stored but that my memory-image of those I saw last spring *is* stored? One may then ask whether there is any better scientific evidence for the latter than for the former. M. Bergson is unwearied in his reiteration of the absurdity of supposing that images are stored in the brain. But M. Bergson contends that memory-images *are* stored in the 'obscure depths' of a realm of being quite disparate from that of the brain. All that one has ever experienced is thus retained. 'I believe,' says M. Bergson, that 'our past life is there, preserved even in the minutest detail; nothing is forgotten; all that we have perceived, thought, willed, from the first awakening of our consciousness, persists indefinitely. But the memories which are preserved in these obscure depths are for us in the state of invisible phantoms.' If this is to be accepted as 'scientific truth' the man of science may reasonably ask for such evidence as he is accustomed to demand in other branches of scientific inquiry. And if it is part of the metaphysics which we are 'to superpose upon scientific truth' this should be more clearly stated than some at least of M. Bergson's disciples are wont to state it. At all events the status of images, ideas, wishes and thoughts in the unconscious—nay deeper than that whether *as such* they are there

existent at all—is perhaps the most important of the fundamental questions which psychology has just now to answer.

And the answer must be sought, not only by those psychologists who have wide training and all-round experience, but in the full light of science as a whole. Part of my aim has been to lay stress on the solidarity of scientific inquiry. The psychologist must not work independently of the physiologist and the biologist, nor they independently of the chemist and the physicist. No member of the brotherhood of science may ignore or contravene what has been established in other fields of research. Though there is more at any higher level of emergent evolution than there is in the lower, the more is never divorced from the less on which it is founded. At the higher stage new modes of relation may obtain; but they are nowise discrepant with those which still obtain in the lower. And we must never interpret the lower in terms which belong to a higher emergent stage. That is false method in science. It is perhaps the cardinal principle of explanation in metaphysics; but in science it must be unreservedly condemned.

We here touch the quick of the world-problem under the interpretation of science and explanation by metaphysics. Emergent evolution works upwards from materiality through life to consciousness which attains in man its highest reflective level. It accepts the 'more' at each stage as that which is given, and accepts it to the full and ungrudgingly. It urges that the 'more' of any given stage is dependent on, or implies, the 'less' of the stages which are prior to it both logically and historically. It does not interpret the higher in terms of the lower; for that would imply denial of the emergence of those new modes of natural relatedness which characterise the higher and make it what it is. Nor does it explain the lower in terms of the higher. It leaves that kind of explanation to metaphysics. If physical changes are explained in terms of life; if physiological changes are explained in terms of unreflective or perceptive cognition, or this is explained in terms of the reflective consciousness which is emergent in philosophical thought; if all that we know is explained as the expression of yet higher and more completely integrated Mind or Knowledge—*that* is, I believe, the distinguishing mark of metaphysical as contrasted with scientific method. I do not deny its validity within its proper sphere. I do question its validity and its utility in science. But to distinguish is not to separate. It may well be that the methods are not antagonistic but complementary. None the less I seek to bring out as clearly as I can the position as I see it. Interpretation of the higher as founded on the lower (but fuller and richer in the advance of nature) is, I conceive, in accordance with the method of science; explanation of the lower in terms of that which is given only at a higher (and eventually the highest) stage—valid as it may be in metaphysics—must unreservedly be condemned in science.

In dealing with a very difficult problem, in trying to dig down to foundations, in seeking to link up psychology with other branches of science under one consistent scheme of natural development, I have doubtless said many things which call for disagreement and

protest. Many perhaps will not accept the distinction I draw between what I regard as empirical and what I regard as metempirical treatment. I have, however, only dwelt upon it so far as seemed to be necessary to indicate my concept of what science is and what it should seek to do. And though, on this occasion when men of science are gathered together, I hold a brief for the science in whose name we meet, it has been no part of my aim to disparage metaphysical explanation within its proper sphere. I may perhaps be allowed to say that, on a different platform, I should be prepared to defend, to the best of my ability, the Creative concept as nowise antagonistic to that of emergent evolution. I should then ask with Kant: 'May it not be that while every phenomenal effect must be connected with its cause in accordance with empirical causation, this empirical causation, without the least rupture of its connection with natural causes, is itself an effect of a Causality that is not empirical but [as Kant puts it] intelligible?'

THE PRESENT POSITION OF THE THEORY OF DESCENT, IN RELATION TO THE EARLY HISTORY OF PLANTS.

ADDRESS BY

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It has long been evident that all those ideas of evolution in which the older generation of naturalists grew up have been disturbed, or, indeed, transformed, since the re-discovery of Mendel's work and the consequent development of the new science of Genetics. Not only is the 'omnipotence of Natural Selection' gravely impugned, but variation itself, the foundation on which the Darwinian theory seemed to rest so securely, is now in question.

The small variations, on which the Natural Selectionist relied so much, have proved, for the most part, to be merely fluctuations, oscillating about a mean, and therefore incapable of giving rise to permanent new types. The well-established varieties of the Darwinian, such as the countless forms of *Erophila verna*, are now interpreted as elementary species, no less stable than Linnean species, and of equally unknown origin. The mutations of De Vries, though still accepted at their face value by some biologists, are suspected by others of being nothing more than Mendelian segregates, the product of previous crossings; opinion on this subject is in a state of flux. In fact, it is clear that we know astonishingly little about variation.

My friend Dr. Lotsy, indeed, proposes to dispense with variation altogether, and to find the true origin of species in Mendelian segregation; inheritable variability, he believes, does not exist; new species, on his bold hypothesis, arise by crossing, and so, as he points out, we may have an evolution, though species remain constant. Thus everything apparently new depends on a re-combination of factors already present in the parents. 'The cause of evolution lies in the interaction of two gametes of different constitution.'

I am aware that very surprising results have been obtained by crossing. Nothing could well have been more striking than the series of *Antirrhinum* segregates which Dr. Lotsy showed us some years ago at a meeting of the Linnean Society. And now we hear of an apetalous *Lychnis* produced by the crossing of normally petaloid races. We do not know yet to what extent that sort of thing goes on in Nature, or what chance such segregates have of surviving. Still, if one may judge by Dr. Lotsy's experimental results, ample material for Natural Selection to work on might be provided in this way.[1]

[1] See Dr. Lotsy's book, *Evolution by Means of Hybridisation*, The Hague, 1916.

Dr. Lotsy's theory that new species originate by Mendelian segregation, if true, would have the advantage that it would make quite plain the meaning of sexual reproduction. Hitherto there has been a good deal of doubt; some authorities have held that sexual reproduction stimulated, others that it checked variation. But, if we eliminate variation, and rely solely on the products of crossing, we get a clear view—'species, as well as individuals, have two parents'; sexual reproduction can alone provide adequate material for new forms, and can provide it in unbounded variety.

Again, though Dr. Lotsy himself is far from sanguine on this point, the crossing theory might be helpful to the evolutionary morphologist, for breeding is open to unlimited experiment, and we might hope to learn what kinds of change in organisms are to be expected. For example, the *Lychnis* experiment shows how easily a petaloid race may become apetalous. Such results might ultimately be a great help in unravelling the course of evolution in the past. We should gain an idea of the transformations which might actually have taken place, excluding those which were out of the question. At present all speculation on the nature of past changes is in the air, for variation itself is only an hypothesis, and we have to decide, quite arbitrarily, what kind of variations we think may probably have occurred in the course of descent. One need only recall the various theories of the origin of the seed from the megasporangium to realise how arbitrary such speculations are.

But, while recognising certain advantages in the theory of the origin of species by crossing, it is not for me to pronounce any opinion as to its truth. It is only the present position of the question that concerns us to-day. We shall hope to hear a statement of Dr. Lotsy's views from his own lips.

Some modern geneticists believe that there is evidence for mutation by the loss of factors, apart from the effects of crossing. Dr. Lotsy considers that such changes, if proved, can afford no explanation of progressive evolution. 'Evolution by a process of repeated losses is inconceivable.' It has, however, been pointed out by Dr. Agnes Arber, in her recent admirable book on Water-plants, that, on any theory of evolution, 'what organisms have gained in specialisation they have lost in plasticity.' She avails herself of a human analogy and says: 'The man, though superior to the baby in actual achievement, is inferior to it in the qualities which may be summed up in the word "promise," just as the Angiosperm, though its degree of differentiation so greatly exceeds that of the primordial protoplasmic speck, is inferior to it when judged by its power to produce descendants of widely varying types' (p. 335).

This is true, but it is not clear that this admitted loss of potentialities is the same thing as the loss of factors, in the sense of genetics. For example, if a glabrous variety of Violet really arose as a mutation by loss of the factor for hairiness, assuming that such a loss was permanent, the effect would seem to be a diminution of specialisation, though, no doubt, it might also be interpreted as a loss of potentiality.

Turning for a moment to Darwin's own theory of the origin of

species by means of Natural Selection, the efficacy of the latter, in weeding out the unfit, is, of course, still acknowledged, and some geneticists allow it a considerable rôle. But there is a strong tendency in these days to admit Natural Selection only as a 'merely negative force,' and as such it has even been dismissed as a 'truism.' Now Darwin's great book was most certainly not written to enunciate a truism. He regarded Natural Selection as 'the most important, but not the exclusive, means of modification' ('Origin of Species,' p. 4). It was the continual selection of the more fit, the 'preservation of favoured races,' on which he relied, and not the mere obvious elimination of the unfit, and this great idea (so imperfectly understood by many of his contemporaries and successors) he worked out with astonishing power, in the light of the changes which man has produced, with the help of his own artificial selection.

It may be that the theory of Natural Selection, as Darwin and Wallace understood it, may some day come into its own again; certainly it illuminated, as no other theory has yet done, the great subject of adaptation, which to some of us is, and remains, the chief interest of Biology. But in our present total ignorance of variation and doubt as to other means of change, we can form no clear idea of the material on which Selection has had to work, and we must let the question rest.

For the moment, at all events, the Darwinian period is past; we can no longer enjoy the comfortable assurance, which once satisfied so many of us, that the main problem had been solved—all is again in the melting-pot. By now, in fact, a new generation has grown up that knows not Darwin.

Yet Evolution remains—we cannot get away from it, even if we only hold it as an act of faith, for there is no alternative, and, after all, the evidence of Palæontology is unshaken. I have thought it fair to lay stress on the present state of uncertainty in all that concerns the origin of species. On another occasion I even ventured to speak of the return of 'pre-Darwinian chaos.' But out of this chaos doubtless light will come.

Last year we had a joint discussion on Genetics and Palæontology; among many good speeches, I specially remember a remark by Miss Saunders, our then President, that Mendelism is a theory of heredity, not of evolution—a caution not unneeded, though, as the crossing hypothesis shows, the connection between the two conceptions may prove to be a very close one.

Genetics is rendering the greatest service to Biology generally in ensuring that organisms shall be thought of as races, not as isolated individuals, mere chemical and physical complexes, at the mercy of the environment. The whole tendency of modern work is to show that in living things Heredity is supreme. An organism is what it is by virtue of the constitution of the germ-plasm derived from its parents. As Dr. Church has said in one of his recent Botanical Memoirs: 'The individual is no longer to be regarded as an isolated unit, or a casual creation, but is the present representative of a "race." That is to say, the individual is not, as short-sighted chemical physiologists tend to believe, a mere physical mechanism, the creature of the external

environment to which it passively responds; but it is the living presentation of a continuous line of organism, successful since living, or a "race" leading back as the expression of continued response to very similar, but not necessarily identical, environment, in unbroken plasmatic continuity, over a period of time which, in terms of ultimate cytological history, may represent a continuous reaction and record for anything up to such an inconceivable period as two thousand million years.' This expresses the case vigorously, whether we accept the time estimate or not. Dr. Church goes on to say that 'during this period the more fundamental reactions, as expressed in morphological units of construction, have been established as constants beyond any hope of change.'[2] This last statement is an important one for the palæontologist, for all our attempts to trace descent rest on the assumption that, in a general sense and as regards certain well-established characters, 'Like breeds like.'

History, then, broadly speaking, is everything. But there is more than one kind of history in Biology. First, we have the exact records of the Mendelian from generation to generation, F^1, F^2, and so on; this alone is adequate, but we usually have to be content with something much less. At the other end of the scale there is the fossil history, full of gaps and uncertainties of every kind, but always imposing from its vast duration. Then there are intermediate kinds of biological history, such as the imperfect records of the breeding of cultivated plants or domestic animals. These can sometimes now be interpreted in the light of the more exact genetic histories, as Dr. Lotsy will show us in the case of some neglected and misinterpreted observations of Darwin. 'Domestication,' as he says, 'spells segregation, followed by selection and isolation of the desirable segregates.' Darwin himself, though necessarily groping in the dark where genetics were involved, yet thought the study of cultivated and domestic races the best clue to the origin of species. If this holds good still, it makes a strong point in favour of the crossing theory of evolution, for the history of cultivated races seems to be largely the history of deliberate or unconscious Mendelian crossings. We may reasonably expect to find a relation between the process of origination of new cultural races and that of new species in Nature.

This suggests the question, what we mean by a 'species'—far too difficult a matter to discuss now. Whatever we may think of Darwin's theory, his 'Origin of Species' is at any rate a classic, and I believe we cannot do better than continue to use the word in the same sense as Darwin used it—*i.e.* essentially in the sense of a Linnean species.

Perhaps the best answer to the question 'What is a species?' is in the form '*Ranunculus repens*,' avoiding all attempts at definition. I know Dr. Lotsy thinks differently, but pure races, whatever else and however important they may be, 'are but rarely or never met with in Nature' (Lotsy), and are certainly not *species* in the classical sense in which Darwin used the word; to my mind it seems a pity to go out of our way to change completely the meaning of a familiar term. We

[2] *Form Factors in Coniferæ*, Oxford, 1920, p. 22.

can continue to call 'pure races' by that name or any more modern equivalent, and 'elementary species' may still be called so, or I have no objection to calling them 'Jordanons.' In the interests of practical taxonomy they necessarily have to be kept subordinated to Linnean species. There are difficulties enough either way, but they are, as it seems, less if we adopt the conservative course. That many Linnean species are real units of a definite order is generally admitted. Dr. Lotsy himself dwells on their distinctness, which depends on their usually not inter-crossing, and appears to be shown by the fact that among animals members of the same species recognise each other as such and habitually breed together. Such habitual breeding together under natural conditions is perhaps the best test of a species in the Linnean sense. 'The units within each Linneon (=species) form an inter-crossing community.' (Lotsy.) He adds: 'Consequently it is Nature itself which groups the individuals to Linneons.' These 'pairing communities' have recently been re-christened by Dr. Lotsy 'syngameons,' [3] a good name to express this aspect of the old 'species.'

I do not propose in these brief remarks to venture on that well-worn subject the inheritance of acquired characters—*i.e.* of such characters as are gained during the lifetime of the individual by reaction to the environment. There has always been a strong cross-current of opinion in favour of this belief, especially, in our own time, in the form of 'unconscious memory,' so ably advocated by Samuel Butler and supported by Sir Francis Darwin in his Presidential Address to the British Association at Dublin. Professor Henslow, as we all know, is a veteran champion of the origin of plant structures by self-adaptation to the environment. On the other hand, some geneticists roundly deny that any inheritance of somatically acquired characters can take place. In any case, the evidence, as it seems, is still too doubtful and inadequate to warrant any conclusion, so, however fascinating such speculations may be, I pass on.

To bring these introductory remarks to a close, we see that while the theory of Descent or Evolution is undisputed, we really know nothing certain as to the way in which new forms have arisen from old. During the reign of Darwinism we commonly assumed that this had happened by the continual selection of small variations, and we are no longer in a position to make any such assumption.

We have been told on high authority that 'as long as we do not know how *Primula obconica* produced its abundant new forms it is no time to discuss the origin of the Mollusca or of Dicotyledons.' (Bateson.) Yet this is just the kind of speculation in which a palæontologist is apt to indulge, and if kept off it he would feel that his occupation was gone! However, so long as we may believe, as already said, that, on the whole, like breeds like, that grapes do not spring from thorns or figs from thistles, there is perhaps still sufficient basis for some attempt to interpret the past history of plants in terms of descent. But certainly we have learnt greater caution, and we must be careful

[3] Lotsy, *La Quintessence de la Théorie du Croisement.* Archives Néerlandaises des Sciences, Sér. III. B., t. iii., 1917.

not to go far beyond our facts, and, in particular, to avoid elaborate derivations of one type of structure from another where the supposed transitional forms have but a purely subjective existence; we have realised the difficulty of tracing homologies. We may still be allowed to seek affinities, even where we cannot trace descent. And though we may sometimes go a little beyond our tether and give rein to bolder speculations, there is no harm done so long as we know what we are doing, and there may be even some good in such flights if our scientific use of the imagination serves to give life to the dry bones of bare description. On this subject I am somewhat more optimistic than Dr. Lotsy, who, abandoning his 'Stammesgeschichte' point of view, has dismissed all attempts at phylogenetic reconstruction as 'fantastic.'

There are some questions of the highest interest that at present can scarcely be approached in any other but a speculative way. Within the last year or two new points of view have thus been opened out. For example, Dr. Church's able essay on 'Thalassiophyta and the sub-aerial transmigration' has brought vividly before us the great change from marine to terrestrial life.

The origin of a Land Flora had, of course, been discussed with much ability before, but rather as incidental to a morphological theory. Dr. Church puts the actual conquest of the land in the foreground. We watch the land slowly rising toward the surface of the primeval ocean, the rooted sea-weeds succeeding the free-swimming plankton, and then the continents slowly emerging and the drama of the transmigration as the plants of the rock-pools and shallows fit themselves step by step for sub-aerial life when the dry land appears. It is a striking picture that is thus displayed to our view—whether in all respects a faithful one is another question; we must not expect impossibilities. The doubts which have been raised relate first to the assumed world-wide ocean, which seems not to be generally accepted by geologists. If continental ridges existed from the first (*i.e.* from the original condensation of watery vapour to form seas), the colonisation of the land may have followed other lines and have happened repeatedly. Perhaps, after all, that would not greatly affect the botanical aspects of the transmigration.

The other difficulty is, however, a botanical one. Dr. Church looks at the whole problem from the sea-weed point of view, and it is well he does, for sea-weeds have been badly neglected, especially by some of the great continental morphologists, who used to lead our speculative flights. Dr. Church is much impressed by the high organisation of many sea-weeds, especially, in the living marine flora, by that of the Brown Algæ. Here we find well-differentiated leaves, special reproductive shoots, extremely efficient holdfast roots, and, sometimes, a definite alternation of generations, while, on the anatomical side, we meet with true parenchymatous tissues, a well-developed phloëm and secondary growth in thickness. There is, in fact, in many respects, an anticipation of, or an analogy with important features which characterise the higher plants of the land.

Dr. Church believes that the chief morphological characters of the Land Flora were first outlined in the sea; that such characters were not

newly assumed after transmigration, but that they merely represent an adaptation to sub-aerial conditions of a differentiation already attained at the phase of marine phytobenthon (rooted sea-weeds). At the same time it is not suggested that any existing class of sea-weeds can be taken as representing the ancestry of the Land Flora; the transmigrant races are, as Algæ, extinct—they may have been Green Algæ of a high grade of organisation, on a level now perhaps most nearly represented by the highest of the Brown Seaweeds.

Thus the transmigrants, which were destined to become the parents of the Land Flora, are pictured as already highly organised and well-differentiated plants, which only needed to provide themselves with absorptive instead of merely anchoring roots, and with a water-conducting system (xylem and stomata) in order to fit themselves for sub-aerial life, while, on the reproductive side, the great change remaining to be accomplished was the adaptation of the spores to transport by air instead of by water.

It is clearly impossible to criticise the theory in detail, for the assumed transmigrants are *ex hypothesi* unknown; we can only form a distant conception of what they were from the analogy of the highest sea-weeds of the present day, which admittedly belong to quite different lines of descent. Dr. Church puts the transmigration so far back (pre-Cambrian) that not much help can be expected from fossils, but to this subject we shall return.

Some botanists find a difficulty in accepting the suggestion that plants already elaborately fitted out for a marine life could have survived the transition, however gradual, to a totally different environment. Such thinkers prefer to believe that lower forms may have been more adaptable, and that morphological differentiation had, in a great degree, to start afresh when the land was first invaded. My own sympathies, I may say, are here with Dr. Church, for I have long inclined to the belief that the vascular plants were, in all probability, derived from the higher Thallophytes. The view of the late Professor Lignier, now so widely accepted, that the leaf, at least in the megaphyllous or Fern-like Vascular Plants, was derived from specialised branch-systems of a thallus, assumes, at any rate, that the immediate ancestors possessed a well-developed thallus, such as is now known only among the higher Algæ. The Hepaticæ, as we now know them, clearly do not come into question, and the Pro-hepatics, which Lignier postulated as early ancestors, have only a theoretical existence, and if they were ever present in the flesh may well have been transmigrant Algæ.

The question now arises, how far have we any evidence from the rocks, which may bear on the transmigration and on the nature of the early Land Flora? A very few years ago no such evidence was available—such data as we then possessed seemed too obscure to discuss. Quite recent discoveries, especially those from the famous Rhynie Chert-bed, have shown that in Early Devonian times certain remarkably simple land-plants existed, which in general configuration were no more advanced than some very ordinary sea-weeds of the present day. At the same time these plants were obviously fitted for terrestrial life,

as shown by the presence of a water-conducting tissue and stomata, and by the manifestly air-borne spores. These simplest land-plants are the Rhyniaceæ (*Rhynia* and *Hornea*), while the third genus, *Asteroxylon*, was more advanced and further removed from any possible transmigrant type.

My friend Dr. Arber was so impressed by the primitive character of *Rhynia* (the only one of these genera then known) that he boldly called it a Thallophyte, while recognising, in respect of anatomical structure, an intermediate position on the way to Pteridophyta. This is not really very different from the view taken by the investigators themselves, though they call the plants Pteridophytes, which they certainly are, if we go by internal structure rather than external morphology. But if, as Kidston and Lang suggest, the Rhyniaceæ 'find their place near the beginning of a current of change from an Alga-like type of plant to the type of the simpler vascular Cryptogams,'[4] they must have been very primitive indeed and might even be regarded as fairly representing the true transmigrants which had not long taken to the land.

It is true that the Middle Devonian is much too late a period for the original transmigration (I believe there is some evidence for land-animals in the Lower Silurian), but one may argue that some of the transmigrant forms may have survived as late as the Devonian, just as the *Selaginella* type seems to have gone on with little change from the Carboniferous to the present time. There must have been many such survivals of earlier forms in the Devonian period, if Arber was right in regarding all the characteristic plants of the *Psilophyton* Flora as 'much more probably Thallophyta than Pteridophyta.'[5] Certainly some of them, apart from the Rhyniaceæ, have an alga-like appearance (e.g. *Pseudosporochnus*) and there is some evidence that such plants also were already vascular. There is, in fact, no doubt that the earlier Devonian Flora is turning out to have been on the whole more peculiar and more unlike the higher plants than we realised a few years ago. The Early Devonian plants cannot usually be referred to any of the recognised groups of Pteridophytes, and this is not owing to our imperfect knowledge, for it is just in those cases where the plants are most thoroughly known that their unique systematic position is most manifest. Arber called all the plants in question 'Procormophyta'—an appropriate name. As Kidston and Lang point out in their later work, the three groups—Pteridophyta, Bryophyta, and Algæ—are brought nearer together by the Rhynie fossils.

And yet there is evidence that about the same period stems with the highly organised structure of Gymnospermous trees already existed. I refer to remains of which *Palæopitys Milleri*, from the Middle Old Red Sandstone of Cromarty, is the type. We need much further investigation of these higher forms of Early Devonian vegetation, but we know enough to impose caution on our speculations.

[4] This view is further developed and expanded in the authors' fourth memoir, which I have had the privilege of reading in MS.

[5] *Devonian Floras, a Study of the Origin of Cormophyta.* Cambridge, 1921, p. 47.

The Rhyniaceæ, at all events, were leafless and rootless plants. In one species of *Rhynia* and in *Hornea* the aerial stems are entirely without any appendages, while in the other *Rhynia* there are hemispherical swellings, which have been identified by Arber with certain states of the spines in *Psilophyton*. The emergences of *R. Gwynne-Vaughani* have been interpreted as nascent leaves, but more recent observations, showing their late histological origin, have rendered this hypothesis very doubtful.

In *Asteroxylon*, a higher plant altogether, the stem is clothed with quite distinct leaves, though they are somewhat rudimentary as regards their vascular supply. Have we, in these plants, and others of contemporary date, the first origin of the leaf from a mere non-vascular emergence, or had reduction already begun, so that in Rhyniaceæ, for example, the leaves were in the act of disappearance? In the former case we should be assisting at the birth of Lignier's phylloids, the microphylls of the Lycopod series, though, as just mentioned, the outgrowths in *Rhynia Gwynne-Vaughani* may have had nothing to do with leaves.

But the opposite view may also be tenable. We have already seen that these plants have been referred both to the Pteridophytes and the Thallophytes; they also show signs of Bryophytic affinities, and I understand that it has even been proposed to include them in the Bryophyta, in which case every possible view will be represented. The *Sphagnum*-like structure of the columellate sporangium or sporogonium of *Hornea* and *Sporogonites* may justify the Bryophytic attribution, and it is then, of course, easy to extend it to *Rhynia*. If we were to adopt this opinion, we should probably have to regard these simple Devonian plants as representing stages in the reduction of the sporophyte to a sporogonium, the leaves being already nearly or quite lost, while the branched thallus was still much in excess of the simple seta of the modern Moss or Hepatic. Naturally we know nothing of the gametophyte, so that the material for comparison is limited. Kidston and Lang, however, have recently pointed out that the presence of spore-tetrads clearly indicates the existence of a gametophyte.

I make no attempt to decide between these views. There can be no reasonable doubt that the Psilophytales generally represent an earlier phase of Cormophytic life than any of the groups previously recognised. But we must not assume that *all* their characters were primitive. It has been pointed out that the Rhyniaceæ were peat plants, and that the peat-flora is apt to be peculiar. Under such conditions it is not improbable that a certain amount of reduction may have already been undergone, though this is not the view taken by the investigators.

There is one more point in connection with the Rhynie plants which may be mentioned, as it is of purely morphological interest, and may be more in place here than at a later stage of the discussion.

In *Hornea*, as Kidston and Lang have shown, the terminal 'sporangia evidently arose by the transformation of the tips of certain branches of the plant.'

They are, in fact, very little modified as compared with vegetative

parts of the stem. The epidermis and subjacent layers of the sporangial wall differ but slightly from the corresponding tissues of the branch, while the columella is continuous with the phloëm, and resembles it in structure. The sporangium has no special stalk, and in some cases is forked, like the stem, having evidently been formed when the branch was in the act of dichotomy.

In *Rhynia* the sporangia are better differentiated, but here also cases occur where the spore-bearing region differs little in structure from the branch which it terminates. In both genera the spore-containing organ is thus nothing but the more or less altered end of a branch, quite comparable to the stichidium, which is differentiated in some Red Seaweeds as the receptacle of the tetraspores, while in other Algæ of this group the tetraspores are produced in unaltered portions of the thallus. In *Hornea* the fertile branch-ending is less differentiated than in *Rhynia*, and we must be prepared to meet with related forms in which the spore-bearing region was not differentiated at all, except for the presence of the spores.

Goebel taught that the sporangium was an organ *sui generis*, a special reproductive structure, which had never arisen from any vegetative part of the plant.[6] His view has been generally accepted, but, in the light of the conditions in Rhyniaceæ, appears to be no longer tenable. While the spores may still be described as organs *sui generis*, for there have always been reproductive cells since plants became multicellular, the sporangium proves to be really a portion of the vegetative stem or thallus, which has gradually become specialised as a receptacle for the spores. The sporangium thus turns out to be strictly homologous with a definite part of the vegetative body of the plant. In these remarks I am glad to find myself entirely in accord with the views of Kidston and Lang, as stated in their fourth memoir on the Rhynie plants.

The recent work on the Early Devonian Flora has wide bearings. It has long been noticed that among the fossils of that period no typical Fern-fronds are found. Those remains which are most suggestive of Fern-like habit consist merely of a naked-branched rachis. It used to be assumed that the absence of a lamina might be explained by bad preservation. But, as Professor Halle points out, the chief reason for condemning the preservation as bad was the fact that a lamina was absent!

The evidence really seems to indicate that the so-called fronds of that age did not possess a leaf-blade. As Professor Halle says: 'In the Lower Devonian, finally, we find frond-like structures bearing sporangia, but no fronds with developed laminæ. One can hardly escape the conclusion that the "modified" fertile fronds may represent the primitive state in this case and that the flattened pinnules are a later development, as suggested by Professor Lignier.'[7] These naked

[6] 'Vergleichende Entwickelungsgeschichte der Pflanzenorgane.' Schenk's *Handbuch der Botanik*, Bd. III., Part I., p. 130, 1884.

[7] T. G. Halle: *Lower Devonian Plants, from Röragen, in Norway.* Stockholm, 1916, p. --

fronds may, in fact, be regarded as the little-differentiated branches of a thallus. It is often impossible to say whether we have to do with the ramification of a stem or with a frond. Halle even suggests that one of his species of *Psilophyton*, *P. Goldschmidtii*, may furnish us with an intermediate stage between the two, as required by Lignier's hypothesis. Plants of the *Rhynia* type may represent a still earlier phase, in which there was no differentiation whatever, but merely a branched thallus. It is a curious point that 'the circinate vernation of the Fern-fronds is paralleled in the branches of *Psilophyton princeps*.'

The evidence, as at present understood, seems to suggest that, in the earlier Devonian Flora, Ferns, properly so called, may not yet have been in existence. The predecessors of the Ferns (Lignier's 'Primofilicinées,' *not* Arber's 'Primofilices') were there no doubt, but not, so far as we know, the Ferns themselves. Yet it seems that highly organised stems of a Gymnospermous type were already present at about the same period. Thus the evidence from the older Devonian Flora, so far as it goes, materially supports the opinion that the Seed Plants cannot have arisen from Ferns, for the line of the Spermophyta seems to have been already distinct at a time when true Ferns had not yet appeared.

The idea that the Gymnosperms were derived, through the Pteridosperms, from the Ferns, which I once advocated, must, I think, be given up, on grounds which were stated two years ago at the Bournemouth meeting of the Association. It is safer to regard the Pteridosperms, and therefore the Seed Plants generally, as a distinct stock, probably as ancient as any of the recognised phyla of Vascular Cryptogams, and derived from some unknown and older source. At the same time the striking parallelism between the Pteridosperms and the true Ferns must be recognised. These views are essentially in agreement with those previously expressed by my friend Dr. Kidston.

I may be permitted to quote in this connection an interesting remark made by Professor Paul Bertrand in a letter received last year. He was speaking of a strange group of plants of Lower Carboniferous or possibly Upper Devonian age, the Cladoxyleæ. These plants have a complex polystelic structure in both stem and petiole, but seem to be quite distinct from the later and better-known polystelic family, Medulloseæ. Professor Bertrand, the chief living authority on the Cladoxyleæ, speaks of them as very primitive types, in which the distinction between stem and petiole was still but little marked. Yet he considers them as most probably Phanerogams. These views, if confirmed, imply that the Phanerogams or Seed Plants started as a distinct phylum, quite low down, at a phase when the differentiation between stem and leaf was still incomplete.

Without laying too much stress on an expression of opinion such as Professor Bertrand's, I believe the present evidence is in harmony with the view he suggests. The Spermophytes, as it seems, have been an independent class of plants from very early times; they are not to be derived from the Vascular Cryptogams, as we have hitherto conceived them, but are of the same standing with them, having sprung

from some long-extinct stock, comparable, perhaps, to Kidston's and Lang's Psilophytales, though not necessarily on the same line.

The significance of the Pteridosperms has perhaps been somewhat misunderstood. It now seems that they do not, as some of us once imagined, indicate the descent of the Seed Plants from Ferns, but rather show that the Seed Plants passed through a Fern-like phase; they ran a parallel course with the true cryptogamic Ferns, and, like them, sprang from some quite early race of land plants, such as Rhynie has revealed to us. But the phylum was never any more Fern-like than the Pteridosperms themselves. This, at least, is the view which now suggests itself, but our knowledge is still very meagre. We especially want to know more about the Devonian Spermophyta, for at present we have scarcely any evidence even of the existence of seeds in any Devonian Flora. Such data as we possess are all anatomical, and a disciple of Williamson must be on his guard against the risk of repeating the old mistake of the Brongniartian school.

Having ventured so far into speculative regions, it may be well to return for a moment to the facts, and ask to what extent our knowledge of the Fern-like Seed Plants has advanced since the original discoveries of 1903-1906. I fear that there is not very much to record. We now have one or two additional species of *Neuropteris* bearing seeds, and also the probable seed of *Heterangium*. Further, we have various indications of the characters of the pollen-bearing organs in some Pteridosperm genera, though the documents, being mostly in the form of impressions, are deficient in detail. Such new information as has come to hand confirms in a satisfactory manner our former conclusions, but does little to extend them.

On the anatomical side there has been more liveliness. We now know quite a number of Palæozoic plants, of varied structure, which have something in common with the better-established Pteridosperm families, Lyginopterideæ and Medulloseæ, while they certainly have nothing to do with Lycopods, Horsetails, or Sphenophylls. We therefore call them Cycadofilices or Pteridosperms. I prefer to use one name for them all and incline to the latter, for, while the plants are generally more or less Fern-like in structure, many of them show no special resemblance to Cycads.

At present we know of no fewer than eight families, based mainly on anatomical characters, which we provisionally include under Pteridosperms:

1. The familiar Lyginopterideæ (Lower and Upper Carboniferous).
2. The *Rhetinangium* family, founded on Dr. Gordon's new genus (Lower Carboniferous).
3. The Megaloxyleæ, discovered by Prof. Seward (Upper Carboniferous).
4. The Calamopityeæ, recently enriched by Dr. Kidston with a new genus, besides new species (Lower Carboniferous).
5. The *Stenomyelon* family, another of Dr. Kidston's discoveries, described by him in conjunction with Gwynne-Vaughan (Lower Carboniferous).

6. The *Protopitys* type, a singularly isolated one, elucidated by Solms-Laubach (Lower Carboniferous). The above are all monostelic. Next come the two essentially polystelic groups:

7. Cladoxyleæ, already mentioned, a somewhat mysterious race, of Lower Carboniferous or possibly even Upper Devonian age.

8. The well-known Medulloseæ (Upper Carboniferous).

It is noticeable that five of these families are Lower Carboniferous (or possibly, in certain instances, older); one (Lyginopterideæ) includes both Lower and Upper Carboniferous members, while two (Megaloxyleæ and Medulloseæ) are at present known only from the Upper Carboniferous.

Of the eight families in question there are only two (Lyginopterideæ and Medulloseæ), in which we have any evidence as to the fructification. The other six are known only by their vegetative and mostly by their anatomical features. Of these the Protopityeæ and the Cladoxyleæ are the most isolated, differing, for example, in the structure of their tracheides from the other families. There seems to be no reasonable doubt that the families represented by *Lyginopteris*, *Rhetinangium*, *Megaloxylon*, *Calamopitys*, *Stenomyelon*, and *Medullosa* are related, and belong to one and the same main phylum. Considering that members of two widely separated families in this series are known to have borne highly organised seeds, there is a strong presumption that the whole set were reproduced by seeds of some sort. In the case of the two families Protopityeæ and Cladoxyleæ the marks of affinity are less obvious, but even here there is more in common with the type-families Lyginopterideæ and Medulloseæ than with any other group.

I think then that we are justified, in the present very imperfect state of our knowledge, in provisionally keeping all these families together, as probably, in some wide sense, Pteridosperms. On this view, they formed a distinct, extensive, and varied class of plants, already very well developed in Lower Carboniferous times, and no doubt going back to the Upper Devonian, though here the available evidence is scanty.

The question may be asked: Did all the Seed-plants pass through the Pteridosperm phase, or were there other parallel lines of descent? Some recent work, no doubt, tends to link up the Cordaitales with the Pteridosperms. *Mesoxylon*, for example, is merely a *Cordaites* with centripetal wood in the stem, a character which strongly suggests an affinity with the *Lyginopteris* or *Calamopitys* type. In fact, some members of the Calamopityeæ (Zalessky's *Eristophyton*) show a certain approach to Cordaitales.

A more striking point is that no marked distinction has been found between the seeds of Pteridosperms and those of Cordaitales. The general community of seed-structure is strong evidence of close affinity and of a common stock.

There seems to be no proof that the family Cordaiteæ existed as such in Devonian times; we do not know much about them even in the Lower Carboniferous; the family is typically Upper Carboniferous and Permian. On the other hand, the *Pitys* family, which we include in the wider group Cordaitales, is as old as any known Pteridosperm;

Zalessky's genus *Callixylon*, an evident ally of *Pitys*, is of Upper Devonian age. The affinities of the still more ancient *Palæopitys Milleri* have not yet been determined.

The position of the Pityeæ hangs in the balance, at least until Dr. Gordon's new results are fully placed before us. From his discovery of the peculiar foliage and leaf-traces as well as from the stem-structure it appears that the Pityeæ form a very distinct group, farther from the other Cordaitales than we once supposed, and not much like any of the Pteridosperms either. At any rate, we may suppose that the Pityeæ branched off from the common stock low down; while the Poroxyleæ and Cordaiteæ may have been of later origin. For the present, however, one may be content to regard the early Spermophytes as constituting a single main phylum. Since these words were written, however, Dr. Margaret Benson has maintained a contrary view, arguing that the Cordaitales, Ginkgoales, and Conifers represent a wholly distinct stock, more allied to the Sphenopsida than to the Fern-like[8] races. The independence of this line has also been maintained by Prof. Chamberlain[9] and discussed by Prof. Sahni.[10]

On our hypothesis, the Upper Palæozoic phyla, with which we have to reckon, are the Pteridosperms (representing the early phase of the Seed-plants), the Ferns, the Sphenophylls, the Equisetales, and the Lycopods. These five lines were probably all well differentiated in the Upper Devonian Flora; the only doubt concerns the Equisetales, which seem not to be known with certainty before the Lower Carboniferous, but they were so well developed then that they must have existed earlier.

When we get back to the Middle and Lower Devonian the case is completely altered. Not one of the five phyla is here clearly represented, unless it be the Spermophyta; for these we have the evidence of apparently Gymnosperm-like stems. Thus the field is left absolutely open to speculation. We may imagine, either that the various phyla converged in some early vascular stock (illustrated by the Psilophytales), or that they ran back in parallel lines to independent origins among the transmigrant Algæ and, perhaps further still, to separate races of purely marine plants. Both views are represented in the publications of recent authors.

Dr. Arber, in his 'Devonian Floras,' maintained the early existence of three distinct lines of descent: the Sphenopsida, Pteropsida, and Lycopsida. In agreement with the present writer, he included the Equisetales in the Sphenopsida. Each of the three lines is described as descended from Thallophytic Algæ of a distinct type. Thus Arber's view was decidedly polyphyletic. It must, however, be borne in mind that the supposed ancestral 'Algæ' were plants in which he expected to find 'some form of primitive vascular system, at least as far advanced as in *Psilophyton*' (*l.c.*, p. 74).

8 'The Grouping of Vascular Plants,' *New Phytologist*, June 30, 1921.

9 'The Living Cycads and the Phylogeny of Seed Plants.' *American Journal of Botany*, vol. 7, 1920.

10 B. Sahni, 'On the Structure and Affinities of *Acmopyle Pancheri*.' *Phil. Trans. R. Soc.*, Ser. B., vol. 210, 1920.

Arber derived the Sphenopsida from Algæ-bearing whorled branches of limited growth, converted into leaves, which were originally and always microphyllous. The Pteropsida, with which he associated his Palæophyllales (*Psygmophyllum*, with foliage like the Maiden-hair tree), were descended from Algæ in which the branches were large, numerous, scattered, and not whorled, eventually metamorphosed to megaphyllous leaves. The Lycopsida, on the other hand, were derived from Algæ in which the usually dichotomous axis bore emergences, metamorphosed to microphyllous leaves.

Thus, as regards the origin of the leaf, Arber was in general agreement with Lignier, while he differed from the French author in the important point that he did not derive the Sphenopsida from the Fern-stock, but kept them as an independent line.

A remarkable feature in Arber's hypothesis is his treatment of the Psilotales. He made this problematic family 'a quite independent race, also of Algal origin, which appeared on the scene long after the other races . . . possibly in Mesozoic times or even later' (p. 87). Thus he rejected both the connection with Psilophytales, suggested by Kidston and Lang, and the affinity with Sphenopsida, once maintained by the present writer.

We thus see that, on Arber's view, there were altogether four distinct lines of descent, running back independently to 'Thallophytic Algæ.'

Dr. Church, from quite a different point of view, arrives at somewhat similar conclusions, but he goes further. He says: 'Speaking generally, it appears safer to regard a "race" or "phylum" as the expression of a group of organisms which derived their special attributes from the equipment of a preceding epoch, if not in one still further back. Thus all the main lines of what is now Land Flora must have been differentiated in the Benthic Epoch of the sea (*i.e.* as algal lines), as all algal lines were differentiated in the Plankton phase. The possibility is not invalidated that existing groups of Land Flora may trace back their special line of progression to the flagellated life of the sea, wholly independently of one another (Pteridophyta).' ('Thalassiophyta,' p. 41.)

Taking the Lycopods and Ferns as an example, and arguing from their different types of flagellated spermatozoids, Dr. Church states: 'It appears impossible to avoid the conclusion that the Lycopod phyla only merge with those of the Filicineæ in a distant Plankton phase, even beyond an independent origin as benthic sea-weeds' (*l.c.*, p. 82). Thus the idea of independent parallel lines of descent is carried to its extreme limit. 'Each phylum goes back the whole way, without any connection with anything else.' Of course, this thorough-going polyphyletic conception is involved in the doctrine already mentioned—that morphological differentiation was attained in the sea before the transmigration.

I have cited Dr. Arber and Dr. Church as independent representatives, approaching the question from quite different sides, of the polyphyletic or parallel-phyla hypothesis. The opposite view, of convergent monophyletic races, is also well supported. Some reference has already

been made to Professor Halle's position. After speaking of the possible relation of the *Psilophyton* type to Lycopods on the one hand and Ferns on the other, he adds: 'From this point of view the whole pteridophytic stock would be monophyletic, the Lycopsida and the Pteropsida being derived from a common form already vascular. It would not thus be necessary to assume a parallel evolution of a similar vascular system along two different lines.' (Halle, *l.c.*, p. 39.)

He does not refer to the Articulatæ, of which, it is true, there are only the most doubtful indications in the Lower Devonian rocks. Halle, too, accepts Lignier's view of the two-fold origin of the leaf, from emergences in the Lycopsida, from thallus-branches in the Pteropsida.

Kidston and Lang, in the light of their Rhynie discoveries, regard Halle's survey as 'a fair statement of the present bearing of the imperfectly known facts.' They lay great stress on the synthetic nature of their genus *Asteroxylon*, which they say 'appears to agree with *Psilophyton* in possessing in a generalised and archaic form characters that are definitely specialised in the Psilotales, Lycopodiales, and Filicales.' They add: 'The Geological age and succession of the Early Devonian plants are, on the whole, consistent with the origin of the various groups of Vascular Cryptogams from a common source.[11] We have already referred to the Bryophytic features, which have been recognised in the Rhyniaceæ. Kidston and Lang make use of these to extend their tentative conclusions to the Bryophyta. In concluding their third memoir they say: 'In *Rhynia* and *Hornea* we have revealed to us a much simpler type of Vascular Cryptogam than any with which we were previously acquainted. This type suggests the convergence of Pteridophyta and Bryophyta backwards to an Algal stock. The knowledge of *Asteroxylon* confirms and enriches our conception of a more complex but archaic type of the Vascular Cryptogams, which supports the idea of the divergence of the great classes of Pteridophyta from a common type, and links this on to the simpler Rhyniaceæ' (*l.c.*, p. 675.) The monophyletic view, though stated with appropriate caution, could not be more clearly expressed. It is fully maintained in these authors' later statements.

It is evidently impossible to decide between the two theories in the present state of our knowledge; we are now only beginning to acquire some conception of the vegetation of Early Devonian times. The discovery, however, of the existence at that period of an unexpectedly simple race of vascular plants to some extent favours a monophyletic interpretation, even though we accept with some reserve the wonderful synthesis of characters which *Asteroxylon* appears to exhibit. To some minds, too, the important points in which all existing Pteridophyta, however diverse, agree will still suggest a common origin not too remote. Among such common characters may be mentioned the alternation of generations with the sporophyte predominant; the development both of the spores and the sexual organs; and the histology, especially of the

[11] *On Old Red Sandstone Plants, showing structure, from the Rhynie Chert Bed*, Part III., p. 673. In Part IV. this conclusion is further emphasised, and it is suggested that the Rhyniaceæ are really too simple morphologically to suit the views of either Lignier or Church.

vascular system and the stomata. The community of reproductive phenomena is explained by Dr. Church on the principle that reproductive phases are inevitable and are therefore the same in all phyla. A like explanation may to a certain extent be applicable to somatic features, some of which may be the necessary consequences of the sub-aerial transmigration. Thus a polyphyletic hypothesis may no doubt be justified, but it urgently needs to be supported by further evidence of the actual existence of separate stocks among the earliest available records of a Land Flora.

The study of Fossil Botany has led to results of the utmost importance, in widening our view of the Vegetable kingdom and helping to complete the natural system, to use Solms-Laubach's old phrase once more. One need only mention the Mesozoic Cycadophytes, the Cordaitales, the Pteridosperms, the Palæozoic Lycopods and Equisetales, the Sphenophylls, and now, most striking of all, the Psilophytales, to recall how much has been gained. We have indeed a wealth of accumulated facts, but from the point of view of the Theory of Descent they raise more questions than they solve. In this address I have briefly touched on some of the most general and most speculative problems in the hope of giving an opening for discussion. It might have been more profitable to deal in detail with definite facts of observation, but recent discoveries have brought us face to face with the great questions of descent among plants. However imperfect our data may be, both as regards the method and the course of evolution, the problems suggested, nevertheless, make urgent claims on our attention.

THE PLACE OF MUSIC IN A LIBERAL EDUCATION.

ADDRESS BY

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SOME years ago we were sitting round the fire in an Oxford Common Room. The Dean, who had the evening paper, let his eye fall upon a paragraph of musical criticism, and read it aloud in that tone of polished irony which we all knew to be his accustomed mark of disapproval. It was a harmless paragraph and contained somewhere an innocent technicality—I think 'sub-mediant.' When he had finished, he looked across to the eminent scholar by the fireside and said, 'Of course, you know what a "sub-mediant" is?' To which came the answer, slow, meditating and pious, 'God forbid.'

That is fairly typical of the attitude adopted in those days by scholarship and literary culture toward the sister art. There were, no doubt, at Oxford and elsewhere, some notable exceptions, but in general the erudite world of England regarded music as something outside the scholar's province: something to be enjoyed as a recreation or a pastime, something even to be encouraged with generous rewards and good-humoured praise, as the Squire might dismiss the mummers on Christmas Eve; but as far as any sympathy or insight was concerned there had been very little progress since the time when, as Byron says,—

'John Bull, with ready hand,
Applauds the strain he cannot understand.'

Applause, no doubt, as much as you will—artists live on applause—but as for understanding or even supposing that there was anything to be understood—'God forbid.'

Two other remarkable pieces of evidence may be adduced from more recent years. The Home University Library, issued by an enterprising publisher and controlled by a body of very distinguished editors, set out to supply a series of monographs on all subjects in which an intelligent reader could take an interest—science, history, poetry, politics, foreign travel—all were to be included, nothing human was to be alien from it. When, at the completion of the hundredth volume, it was pointed out that there had been no book on Music or on any subject in which Music could enter, the reply was that this omission was intentional for fear there should be no readers. Music was not regarded as one among the hundred subjects most likely to engage a reader's attention. There is a similar omission from the Cambridge History of Literature, that monumental work—*ære perennius*—which has become indispensable to every scholar of our

language or our letters. In it we have criticisms of books of almost every conceivable variety of topic, there is even sympathetic mention of books on pugilism, but there is no account of any books on Music. To emphasise the omission, Burney and Hawkins are both noticed, one as the father of Madame d'Arblay, the other as a rather eccentric member of Johnson's circle, but there is nothing to indicate that they wrote two great historical works which are still read with pleasure and consulted with profit. Everyone who has looked into the matter will have observed this same neglect in bibliographies and dictionaries, and other works of reference. Information about Music and Musical Literature must be sought as a rule in specialised volumes intended for musicians alone. It shares, no doubt, the all-embracing hospitality of the Encyclopædia Britannica, but it has not yet won citizenship in the daily life and civilisation of our people.

This is clearly an error, the perpetration of which is a serious loss to the country at large. Music is not only a source of noble pleasure —everyone admits that, at any rate in theory—it is a form of intellectual and spiritual training with which we really cannot afford to dispense. It is not merely a matter of pleasing the ear with successions of beautiful sound or stirring the emotions with vibrating tone and poignant rhythm. It is just as truly a language as French or Latin. It is just as truly a form of mental discipline as any subject in Science or Mathematics. That it can be studied with much more personal enjoyment than some of its compeers may perhaps be maintained; though on this score there is very little difference between it and literature; but even if that be granted it is a very peevish asceticism which would, for this reason, depreciate its value in our educational system. The notes in a perfect melody follow each other by as sure logical necessity as do the words in a line of Shakespeare. They are not only beautiful; they not only appeal to the discerning ear by a thousand tones and associations; they have also an inherent significance which in music, as in poetry, is a sure criterion of the difference between good art and bad.

No doubt there is here one salient difference between the two arts. In language a part at any rate of the significance depends upon the relation between the thing said and an external reality which it expresses or depicts; in music the whole significance is intrinsic, determined by the laws of its own form and the impulse of its own spirit. But though the kinds of significance are different, the fact of significance is equally present in both arts, and here I would venture to call in question two opinions, both of which seem to me entirely and fatally erroneous. One, which I saw a few days ago, in a volume of essays (and which, indeed, a reader of literary criticism may see almost once a week), is that poetry appeals to the intelligence and music to the emotions. The answer to this is that if poetry is to be summed up as an appeal to the intelligence, then Euclid was a very great poet; and if music has no further function than to appeal to the emotions, then it is nothing better than melodious nonsense. The other of the two is that any succession of notes constitutes a melody and that of such melodies intelligible music can be made up. The answer to

this is that such a sequence of notes can no more make a melody than a sequence of words makes a sentence. Everything depends on whether the words do or do not carry a meaning. Suppose, for instance, I wrote a sonnet of which the last line should run

And purple decks the fragrant empyrean,

I should have produced a sequence of quite admirable words, but it would not be a line of poetry. In just the same way the difference between a melody of Beethoven and the types of melody which once fell under the censure of Sir Hugh Allen is very largely that the melody of Beethoven has a noble meaning, and that the bad tunes of the streets have either an ignoble meaning or none at all. It may frankly be admitted that a vast proportion of what is printed and sold as music is far below this criterion; it is meaningless and therefore worthless. But if the advocates of literature or of the representative arts feel any inclination to despise music on this score they may be recommended, before pronouncing judgment, to look at home. The present generation of English readers has bought 130,000 copies of 'The Young Visiters,' the last generation made the fortune of Mrs. Henry Wood, its predecessor of Martin Tupper, and so the tradition stretches back through T. H. Bayly, Robert Montgomery, and a whole series of false idols surfeited with undiscriminating incense. The state of pictorial art in this country may be attested by some of our print shops, many of our private collections, and most of our municipal galleries. Indeed, it is not from the poet or the artist that one usually hears this argument. They know too well of what slender glass their houses are built. In all arts alike the work which endures is the work which appeals to the whole nature of man, spiritual, intellectual, emotional, and of this there is plenty in music to give full justification to its claim.

Here an objection may be lodged—it may be said that this is merely special pleading, that music would not have been neglected unless it had deserved neglect; and in this there is a great measure of truth. The case for music has been badly presented; a great many hearers who really understand it are no more conscious of the fact than M. Jourdain knew that he was talking prose, and the vast majority who accept it without understanding do so because they vastly overrate its difficulties and are repelled by some unnecessary formalities in its method.

A good many treatises on music correspond not to the writings of literary critics, but to elementary school books on grammar; they are concerned with alphabets and case endings and rules of syntax. The reader who takes them in hand is likely to fling them aside with the same impatience with which Montaigne dismissed the 'trash-names of grammar' which learned men had assigned to 'the tittle-tattle of his chambermaid.' It is not that the technical terms in music are any worse than those in other arts and sciences; they are less aggressive than the botanical description of a rose, and shorter by several syllables than the usual designation of a chemical compound, but they somehow seem to have occupied more of the field. They have forced themselves needlessly upon our attention; they have correspondingly led people to

believe that all musical criticism springs from their tangled roots. And to this may be added a real difficulty which music specially has to confront. Our ordinary language has been so framed with reference to external nature and the life of man that the critic of literature or painting has a far easier task than the critic of musical style or musical structure. Music is equally philosophical in basis, but its philosophy is in the nature of the case, if not more penetrating than that of the poet, a little more abstract in form. Compare, for instance, a play of Shakespeare with a symphony of Beethoven. The comparison is really extraordinarily close; there is the same kind of architectonic power in the construction; there are the same points of interest and adventure; there is the same high and noble emotion; there is the same humour; there is even, allowing for the difference of medium, the same characterisation. But when Mr. Bradley analyses for us a Shakespeare play, there are a thousand points on which he can illustrate his meaning in words and phrases which directly relate his experience to human life. When Sir George Grove analyses a symphony of Beethoven, he is hard put to it to find any verbal analogues at all; when they do come they hardly seem more convincing than metaphors, and almost every point in his admirable account has to be illustrated and enforced by musical examples which the majority of people persistently declare themselves unable to read. The result is that the musical critic has often to substitute emphasis for persuasion, and has tended to dogmatise—not because he is unsure about his convictions (though this is a common basis of dogmatism)—but because the difficulty of expressing them drives him to an unusual trenchancy.

Another reason for the prevalent error is that musical history has been far too sharply separated from the general history of civilisation. This, again, is a matter of proportion; the English Histories of my boyhood were mainly occupied with battles and treaties, and paid very little attention to letters or science or discovery, but at worst they have nothing to show parallel to Lord Macaulay's great History of England which in an exhaustive account of the reign of James II. finds no room for the mention of Purcell. The result is again the loss of human interest which tends to relegate music into a remote and abstract world which is far away from men's business and bosoms. Professor Dowden once wrote a very remarkable essay about the influence of the French Revolution on English literature; an essay of equal interest and importance might be written about its influence on Viennese music. And indeed we are coming more and more to see that the whole artistic expression of the people is an index of its national character and a symptom of its national health.

It is not, therefore, because music is unworthy of a place in our intellectual life that we have hitherto left it so much on one side. We have been frequently reminded of late—and we cannot be reminded too often—that the one supreme period of English music, the period in which our composers stood in the forefront of the whole world, is the period which produced the great Elizabethan seamen and the great Elizabethan dramatists; the period in which Drake circumnavigated the world and Shakespeare the soul of man; and, what is more, that

our madrigal writers and Church writers, and writers for the Virginals and the Lute were not isolated phenomena, brought by some unexpected Providence into a country unfit to receive them; they were the natural outgrowth of a civilisation which accepted music as an essential part of a man's upbringing and nurture. In the days of Elizabeth the whole of England was full of music, as Shakespeare's plays are full of it, and we are not so much better than our Elizabethan ancestors that we can afford to disregard what they claimed as one of the most valuable parts of their education.

It has been said that complaints against an abuse have usually been most urgent at the time when the abuse is in the natural course of being redressed, and this is certainly true of the strictures which have been made in the earlier part of this paper. During the last twenty years an extraordinary change has taken place in the part assigned to music in our civilised life. The reform is only just at its beginning, but it has as a matter of fact begun, and though we may be like Caesar and 'think nought done while aught remains to do,' we can, at any rate, see round us enough signs of progress to go forward with considerable encouragement. For one thing, the study of music no longer means, as it did a generation ago, a reluctant drill in the elementary practice of a musical instrument. We are learning the wisdom of confining our executants to those who show some taste or aptitude for performance, and have come to see that confining the study of music to them is just as irrational as it would be if we confined the study of literature to students who aimed at being poets or actors. By all means develop and encourage our specialised schools of music. They have a great tradition; they have done and are still doing magnificent work, and one of the results which they have already produced is that we are no longer obliged to look to our Continental neighbours for executive and creative artists, that our own players and singers and our own composers can hold their own against any rival in the world. But still more important, and at any rate more germane to this present paper, is the recognition of music as an essential part of that liberal education which we are endeavouring to bestow upon all citizens throughout the country, and it is in this that the most remarkable advance is now being made. Our public schools, which half a century ago treated music as an unpopular alternative to cricket, have now begun to find a place for it, if not always in the curriculum, at any rate in the corporate life. The old days of the visiting music master, shy, embarrassed, probably a foreigner, ill at ease in Common Room, hardly counting as a member of the staff, have now been replaced by a more genial and hospitable system, by which the school music is placed in the hands of a well-educated and genial colleague who can mix with his fellows on equal terms and is as sure of a welcome as any among them. The school concerts are more numerous and of far higher quality than they were in the old days, and in many schools they are prefaced by explanatory lectures on the more elaborate or recondite works performed. Most of all, perhaps, is the change noticeable at Oxford and Cambridge, which have become radiating centres of musical activity and are sending out every year

trained men to carry on their tradition through every corner of the land. Yet hardly less in importance is the action which has recently been taken by some county and municipal authorities, who have appointed special Directors of Music to organise the work for all the schools in their area. The improvement already effected by this means is very remarkable, and will be the more conspicuous still as the movement spreads and advances.

What, then, it may be asked, further remains for us to do? The answer may be suggested on the following lines: First, that music should be recognised in our formal education of school and college; that it should be given a place in the curriculum and full recognition in the examination system. It is likely that this proposal will at once arouse an outcry, on the ground that it is adding a new subject to an already overloaded scheme. But, in the first place, I have never known any teacher complain of overloading in regard to his particular subject; and, in the second place, I would suggest that music for the whole school should consist of little more than class singing and an occasional concert or lecture, and that those who have the taste and aptitude for pursuing its serious study should do so in substitution for some other subject. The study of a great composer might be made of as much educational value as that of a great poet. On the other side, the qualities of abstract thinking and of mental construction implied in the study of musical form are closely analogous to those of our natural sciences, and might well be made of the same educational value. It should be quite possible to draw up a syllabus for music which would fit into the existing schemes of school and college work, and which would neither encourage faddists, nor excuse idlers, nor produce that lamentable class of people, not yet quite extinct, who talk emotionally about music without any understanding. Secondly, there should be a great improvement in the place of music in our libraries. Every public library in the country and, if possible, every school and University library, should contain a musical department which includes not only the standard classical compositions, but the first-rate books on musical æsthetics and criticism. There are a great many more of such books than is commonly supposed. Almost every civilised nation has contributed to them, and they range from entertaining volumes of light essays to such profound philosophical treatises as Schopenhauer's book on 'The Platonic Idea.' At present an allusion to music in average society would tend to cut the conversation down to the roots; half the company would feel nervous and uncomfortable, half apprehensive of a dull or pontifical lecture. It ought to be just as possible for people to be well read in music and interested in communicating their ideas about it as they are at present in ordinary civilised society over questions of literature or the representative arts. And this leads to a third point—that the ordinary educated man ought to be trained to read music. The script, though it is not always very rational, is not unduly difficult, and its mastery unlocks the door of a new literature. A very great many of us have only rare and infrequent opportunities of hearing the best music. We have no means of refreshing our memories between recurrent performances, and we therefore lose

a great deal of the effect which they produce. If we learn to read (by which I do not mean to sing or play at sight, but to read silently as one reads a play or a novel) we have added another valuable resource to our intellectual life. Lastly, and as corollary to all these, we all of us need to simplify our attitude towards music. One result which follows from the uncertainty of its position is that it has not yet found its proper bearings. People who have any musical gifts are a little inclined unduly to stress them, because they have a misgiving that their neighbours do not rate music sufficiently high. The outside world, which would be very glad to understand more about music, but regards it as a kind of hieroglyphic or sacerdotal secret, which the profane may not penetrate, is equally reticent because it is afraid to put forward an opinion in the presence of the expert. We want really to pool our knowledge, to concentrate our interests, to develop on this side, as we have on so many others, a sense of comradeship and co-operation, and this can only be done if we are all made free of the company; if our musical education is such that we can meet each other as frankly and openly in this field as educated men are accustomed to do in the discussion of science or poetry. And this we can only do if music is enfranchised in our educational system, if it takes its assured place in the community and is invested with the full rights of intellectual citizenship.

THE STUDY OF AGRICULTURAL ECONOMICS.

ADDRESS BY

C. S. ORWIN, M.A.,

PRESIDENT OF THE SECTION.

For the third year in succession the University of Oxford has been honoured by the selection of one of its resident members for the office of President of the Agricultural Section of the British Association, and on the occasion of the Edinburgh Meeting it may be of interest to recall that historically, at all events, the study of Agriculture and Rural Economy at Oxford takes second place to no university with the single exception of Edinburgh. I am not a scientist in the commonly accepted sense of the word, and nothing but my deep conviction of the need for wider recognition of the importance of the study of economics in connection with agricultural research work could have overcome my reluctance to assume an office in which I have been preceded by such a long line of distinguished men.

It is now about five and twenty years since research and educational work in agriculture began to be developed seriously in this country. Since that date a very great deal of effort has been expended in investigating the forces by which plant and animal life are controlled, and to bring natural science to bear in every way upon the problems of food production. Work along these lines has been productive of most valuable results to the farmer; but at the same time the fact has been overlooked that, when all is said, farming is a business, and if it is to succeed as such it must be carried on with a clear regard for the economic forces which control the industry. So, whilst desiring nothing but the fullest recognition of work in the fields of natural science applied to the investigation of farming problems, I must express without any qualification the view that the equal importance of the study of these economic forces has never been adequately recognised. Educational and research work in agriculture which takes no account of the dominant importance of economics must always be ill-balanced and incomplete, for farming business requires for its proper control a consideration of human relationships, of markets, of transport, and of many other matters which should come within the purview of the economist, as well as, or even more than, a consideration of questions regarding the control of plant and animal growth with which the scientist, in the limited sense of the name, is concerned. No one could wish to deny the need for the close and continual study of the soil and the means by which it can be made to produce more abundantly;

no one could deny the need for research work in problems of animal and plant life. But the main concern of the farmer is to know not so much that which he can *grow* and how best to grow it as that which he can *sell* and how to sell it at a profit. Given the necessary capital and labour, conditions may be contrived under which any soil may be made to produce any crop; but the wisdom or otherwise of embarking upon any particular form of production can be determined only by a study of economic forces. In Bedfordshire, for example, considerable areas of very moderate land are met with given up to a most intensive form of agriculture; but land equally suitable for a similar form of farming may be met with in many other parts of the country which is producing not a tenth part of the value in food products nor employing a tenth part of the capital and labour, whilst at the same time the systems under which it is farmed are fully justified by the results. The reason of the difference, as doubtless everyone realises, is that the land in the former case is so situated that it has access, in the first place, to supplies of organic manures on an abundant scale and at a cheap price, and, in the second place, to markets crying out for its produce, whilst one or both of these facilities are denied to the other areas. In the Chilterns district of Oxfordshire farming a generation ago was mainly directed to the production of corn and meat, and nothing that has arisen out of the work of the investigators along lines of natural science would have called for any radical changes in agricultural policy on these soils. But economic forces, inexorable in their effect, have brought about a revolution, and arable land previously under corn and sheep is now laid down to grass or occupied with fodder crops for the maintenance of the dairy herds which have replaced sheep throughout the area. Again, in the hill districts of England and Wales there occur combes and valleys admirably adapted by soil and climate to the production of potatoes, and the highlands of Devonshire and Somerset may be cited in illustration. In these places, however, in the majority of cases, even though good markets may exist—Somerset, for example, imports potatoes—the lack of transport facilities makes it impossible for the farmers to produce anything which does not go to market on four legs. Coming last to the question of human relationships, we find that it is possible to organise much more intensive forms of agriculture than any of our own, which would be an enormous advantage to a consuming nation like Britain; examples of such are to be met with in varying degrees of intensity in many countries. The Chinese, one reads, have increased production per unit area to an almost incredible extent, and in a lesser degree a similar state of affairs exists in parts of France and in Belgium (so often held up to us in this country as a model of productive capacity which we should strive to emulate). But in all these places the results are only achieved by a prodigal use of labour. The nation gains, no doubt, in the volume of produce available for its consumption, but the individual producer, deprived under this system of the opportunity to apply his manual effort in conjunction with an adequate amount of capital and land, is sacrificed to the consumer's advantage, and is driven to spend himself, year in and year out, for a reward for his toil which the

British worker, with so many alternative openings in more profitable directions available for him under our industrial system, would never for one moment submit to. From what I have read, I imagine that the fact which drove so many Scottish crofters across the seas was much less the selfishness of deer-stalking landlords than the opportunity for exchanging a few acres of rocks and heather in the Highlands for 160 acres of the virgin soil of Canada. People only submit to poor conditions of life when they have no alternative, and one of the most important studies awaiting the investigator of agricultural economics is that of the lines on which to develop the industry so as to give the worker the biggest reward for his toil.

These few illustrations may serve to indicate the over-riding importance of the economic factor in farming just as in any other business. It is a common experience in industry that many scientific and technical processes are possible which are not profitable, and it is in the light of the profit that they leave that all of them must be judged.

Economic conditions are subject to continual change, and the variations may be both sudden and extreme. This makes it the more needful to be continually recording experience and to examine it for the facts that emerge from which to obtain guidance for future policy. Much information is required both for national and individual guidance. Of late years, for example, there has been much advocacy of more intensive cultivation of the soil; it is said that by closer settlement and more intensive methods the production from the land could be much increased. On the other hand, there are those who advocate a development of extensive farming as being the only means by which to attract capital to the land and to pay the highest wage to the worker. Both sides to this controversy can and do produce evidence in support of their views, and some figures derived from a survey made by my colleague, Mr. J. Pryse Howell, will serve to illustrate both. The total area surveyed was 9,390 acres, divided into fifty-two farms of various sizes, and the region was selected by reason of the uniformity of the general conditions. All available data for each holding were collected, and after grouping the farms according to acreage the figures were thrown together and averaged for each group, with the following result:—

PRODUCTION PER UNIT OF LAND AND PER UNIT OF LABOUR FROM HOLDINGS OF VARIOUS SIZES.

Group	No. of Farms in each Group	Average Size of Farms	Average Arable Land per cent.	Altitude	Average Rent per Acre	Average Men per 100 Acres	Sales per Acre	Sales per Man
Acres.				*Feet*	*s. d.*		£ *s. d.*	£ *s. d.*
I. 0–50	5	39	17	341–369	32 10	7·1	11 19 11	168 19 0
II. 50–100	10	78	22	319–384	33 0	6·4	9 19 2	156 2 0
III. 100–150	14	138	21	370–453	27 2	4·2	7 19 1	189 0 0
IV. 150–250	11	201	11·7	330–411	28 4	3·3	7 5 8	222 12 1
V. over 250	12	356	18·0	286–435	26 5	2·6	8 4 4	316 19 0

It will be noted that the conditions under which the farming is carried on in the various groups show no material differences as between one group and another, except in the matter of area. There is a tendency for rent to fall as the size of the holdings increases, but it is not pronounced, and in one case (Group IV.) the percentage of grassland to arable land is considerably higher than in the rest; but, considering the variations which must be expected in the conditions prevailing over any area of fifteen square miles in extent, it may be claimed that in respect of altitude, quality of land, and proportion of arable to grass the holdings in these five groups are fairly comparable. Taking the results as they stand, the fact emerges that employment and production vary inversely with the size of the holding, but that the production per man employed varies directly with the size of the holding. Thus, on the one hand, the advocates of closer settlement and the intensive methods which must necessarily follow if men are to live by the cultivation of small areas of land would seem to be justified in that the results shown by the survey indicate the highest amount of employment and the greatest product-value in the smaller groups. On the other hand, the advocates of more extensive methods of farming can point to their justification in that it is clear that the efficiency of management is greatest in the larger groups if the standard of measurement be that of product-value per man employed.

However, it is clear that either party is drawing conclusions from incomplete data. The efficiency of any farming system can only be judged by an examination of the extent to which all the factors of production are utilised and balanced under it. Each of the assumptions made from the figures above ignores entirely the factor of capital. Land, labour, and capital are all required for production, and the *optimum* system of farm management is that which utilises all three together so as to secure the maximum result from each. If information were available as to the capital utilised in each of the five groups in the survey it might be found that in the smaller groups labour was being wastefully employed, and that an equal number of men working on a larger area of land with more capital, in the form of machinery equipment, would produce an equal product-value per unit of land with a higher rate of output per man employed. Equally it might be found that in the larger groups the use of more labour, or a reduction in the area of land, might produce the same product-value per man with a higher rate of output per unit of land. Obviously there can be no absolute answer to the question of what constitutes the most economical unit of land for farm production. The quality of land in certain cases, and market, transport, and climatic conditions in many more, make it impossible to determine even within wide limits the size of the holding on which the principal factors of production can be employed with maximum effect. Within similar areas, however, and in limited districts, much work can and should be done by agricultural economists to collect evidence on this point for the information of all concerned with the administration of land.

Another matter of the utmost importance to the farmer and to

the public alike, and one which is crying out for investigation on a large scale, is the distribution and marketing of farm produce. Attention has been drawn at many times to the discrepancy between the price realised by the producer and the price paid by the consumer for the same article. In connection with market-garden produce, for example, the Departmental Committee on the Settlement or Employment on the Land of Discharged Sailors and Soldiers stated in their Report (Cd. 8182, 1916) that 'the disparity between the retail prices paid for market-garden produce in the big towns and the small portion of those prices received by the growers is utterly indefensible. It demonstrates a degree of economic waste which would ruin any other industry.' No evidence was published by the Committee as to the facts upon which this conclusion was based, but a recent inquiry made by the Ministry of Agriculture into the prices prevailing at various stages in the distribution of vegetables in London may be quoted in confirmation of it. Figures were collected to show the amount received by the producer, the wholesaler, and the retailers for various classes of everyday garden stuff, with results as shown below.

PRODUCER'S, WHOLESALER'S, AND RETAILERS' PRICES FOR MARKET-GARDEN PRODUCE, JANUARY 1921.

Article	Cabbages, medium grade, per doz.	Cabbages, bottom grade, per doz.	Cauli-flowers, top grade, per doz.	Sprouts, top grade, per 28 lbs.	Turnips, medium grade, per cwt.
	s. *d.*	*s.* *d.*	*s.* *d.*	*s.* *d.*	*s.* *d.*
Producer . . .	0 3	0 2½	3 0	3 6	3 0
Wholesaler . .	1 0	0 9	5 0	—	5 6
Retailers—					
(*a*) Stalls and barrows	2 6	2 0	6 0	—	14 0
(*b*) Suburban shops .	3 0	2 6	8 0	—	14 0
(*c*) Stores and high-class-shops .	4 0	3 0	10 0	14 0	18 8

One has only to glance at the prevailing methods of distribution to realise their wastefulness. The street in which I live contains ten houses, and each day four milk-carts, three bakers' carts, three grocers' carts, and two butchers' carts deliver food to them. Twelve men, horses, and carts, not to mention a host of errand-boys on foot and on cycles, to deliver food to ten families! While we are content with such a loose organisation of distribution as this represents, we must not wonder if the prices received by producers seem disproportionate to those paid by consumers, particularly when the produce partakes of the nature of market-garden stuff, bulky, perishable, and of low value. But apart from the question of methods of distribution, and the advantages to producer and consumer alike which would accrue from some co-operative organisation directed towards the elimination of unnecessary retailers who do no real service to either of them, an investigation of transport and marketing costs would show to what extent they are being exploited by the distributor. The farmer suffers equally with the market-gardener. At the present time I am getting 1*s*. 9*d*. per

gallon for milk sold to a middleman from my farm, and for this milk my wife is charged 3*s*. per gallon. I am selling lamb at 1*s*. 4*d*. per pound for which she is charged 2*s*. 6*d*. per lb., and if the drought had not upset the crop I should be selling potatoes at an equal disparity as between wholesale and retail prices. Can anyone say whether these figures do or do not represent a fair division of total cost as between producer, retailer, and consumer? It may be asserted with confidence that no one can speak with authority upon the subject. The only figures which we have been able to collect at Oxford on the cost of distribution relate to milk, and the most recent that we have are those for the year 1918. In that year in a Midland manufacturing town we found that the distribution costs of a large producer-retailer were as follows:—

		£	s.	d.
Labour	Manual and clerical	1,242	10	2½
	Horse	497	0	9½
Rent		75		0
Sundry purchases, depreciation, general expenses, &c.		430		1
Total cost		£2,244	13	1
Number of gallons of milk distributed		112,833		
Cost of distribution per gallon		4·77*d*.		

Doubtless the conditions have changed since that year, nor is it possible to generalise from a single example; but, nevertheless, the figure for the gallon-cost seems to indicate that both farmer and consumer are suffering in the interests of the distributor, though it is impossible to say without further investigation whether the profit secured by retailers generally is excessive, or whether the difference between distribution cost and the margin out of which it is paid is necessary owing to an excessive number of distributors.

As to the other points named, meat and potatoes, no evidence exists at all, and the position with regard to them and also to milk is only indicated to emphasise the need for a full investigation of the economics of distribution.

At the present time labour problems afford a useful example of the need for further investigation of the economic problems of agriculture. The agricultural industry has been fortunate in that it has escaped the serious labour troubles which have shaken many other industries so badly during the past few years. This has been due in part, no doubt, to the closer personal relations which exist between employer and employed in agriculture than in other enterprises, and in part to the intervention of that often unfairly criticised body, the Agricultural Wages Board, but agricultural employers have also to thank the fact that agricultural labour is difficult to organise. Much controversy in the past would have been avoided, and the possibility of future difficulties could be faced with more confidence, if all the facts relating to labour had been and were being studied over the country generally. The labourer is often blamed for results which are due to the inefficiency of the farmer as a manager. When wages were low it may have been that the labourer was the cheapest machine, but in proportion as his remuneration approaches more nearly to the standard of reward in competing industries, so will the necessity for making his work more

productive be intensified. The value of the output from the farm per man employed is not the only measure by which to gauge the efficiency of the management, but is certainly one of primary importance. A man with a spade can dig an acre of land in about two weeks at a cost to-day of about 4*l.* 10*s.*; a horseman and a pair of horses can plough an acre in about a day and a-half at a cost of about 1*l.* 15*s.*; a farm mechanic on a tractor can break up an acre in about a quarter of a day, and although in the absence of sufficient data the comparison cannot yet be completed by reference to the cost of motor ploughing, it is fairly safe to suggest that when all the factors are considered—speed, less dependence upon atmospheric and soil conditions, as well as actual cost—there will be a still further advantage to be derived by investing the manual worker with the control of mechanical power. Thus it may be that high labour costs to-day are due in many cases less to the inefficiency of labour and more to the inefficiency of management. In a recent issue of *The Times* an agricultural writer expressed the view that if the means existed for determining the proportion of the net returns of agriculture accruing to-day to labour, it would be found that labour was taking any excessive toll of farming results. This view is probably very generally held, and it affords a good example of the misconceptions which may and do arise in people's minds in the absence of exact information upon which to base their assertions. This happens to be one of the questions which have been the subject of investigation at Oxford, though only on the small scale that the means at the disposal of the University has admitted. An investigation was made before the War of the Distribution of the Net Returns of Agriculture as between landlord, farmer, and labour. The net returns are calculated from the net output, and the net output was ascertained by the method followed in the Final Report on the First Census of Production of the United Kingdom, 1907 (Cd. 6320). Under this method the cost of materials at the works is deducted from the value of the output at the works, and the difference constitutes for any industry the fund from which wages, salaries, rent, royalties, rates, taxes, depreciation, advertisement, and sales expenses, and all other similar charges have to be defrayed, as well as profits. The same basis of calculation was adopted in the Report of the Board of Agriculture and Fisheries on the Agricultural Output of Great Britain (Cd. 6277) made in connection with the Census of Production Act, 1906. In applying this measure of net output to the agricultural industry the method is to value the farmer's capital at the beginning of the year and to add to this figure all live and dead stock bought during the year, foods, manures, tradesmen's bills, on-cost and establishment charges, &c., and to deduct the total from the sales during the year added to the valuation of the farmer's capital at the end of the year. Only in the case of the workers is their share of this net output available as net income. The landlord has to incur a considerable expenditure upon the farm in the way of repairs and maintenance, and this must come out of his share of the net output. From an inquiry conducted by the Land Agents' Society in the year 1909 it appeared that about 30 per cent. of the rent received by the landlord is expended by him in repairs, insurance, management, and similar payments neces-

sary to maintain the property in a condition to produce the rent. The farmer, too, may have certain expenses to meet not covered by those deducted in arriving at the net output, and his share of this figure has also to cover some rate of interest on his working capital besides the reward due to him for the exercise of his managerial functions. Thus, in considering the distribution of the profits of agriculture between the three interests concerned, it is necessary to distinguish between net output as defined in the Census of Production and what may be termed the net returns. The net returns are ascertained by deducting from the net output any additional expenses of the business not already allowed for; a sum representing about 7 per cent. interest on the farmer's capital (this figure being based on current rates for money), and one-third of the amount of the rent.

This method for calculating net returns was applied in 1913 to six farms scattered all over the country and differing from each other in almost every way as to systems of management, soil, locality, and so forth, and it was found that the proportions accruing to each of the three interests varied hardly at all, and that it would be safe to say that 20 per cent. of the total was going to the landlord, 40 per cent. to the farmer, and 40 per cent. to labour. Owing to the disorganisation of the work arising out of the War it was not possible to carry on the investigation on each of these six farms, but it was continued in connection with one of them down to the year 1920. This farm may fairly be described as typical of 'average to rather indifferent' conditions. It was a tenant-farm, about a thousand acres in extent, commanding a rent of less than 1*l.* per acre, about three-quarters arable, situated on light to medium land, seven miles from a station, and farmed mainly for production of corn and meat. Taking the above proportions, namely 40 per cent. each to farmer and labour and 20 per cent. to landlord as the pre-war rate of distribution, and calling each of these shares 100, the proportion of distribution between the three interests varied during the following six years as shown below:—

Distribution of the Net Returns from Farming between Landlord, Farmer, and Labour during the years 1913-14—1919-20.

Year	Landlord	Farmer	Labour
1913–14 (Standard)	100	100	100
1914–15	97	104	99
1915–16	94	108	98
1916–17	91	115	94
1917–18	90	111	99
1918–19	87	115	98
1919–20	89	109	102

The figures are interesting in several ways. In the first place they seem to disprove the suggestion referred to above, that labour has been taking an undue share of the net returns from farming, for an examination of the figures in the 'Labour' column shows that until the

institution of the Agricultural Wages Board in 1917 the tendency was in the direction of a slight but steady reduction in the proportion coming to the workers; the effect of the Wages Board Orders was to steady this tendency and, ultimately, to bring labour back approximately to the position it occupied in 1913-14. If the figures could have been continued for another year it is likely that they would show a material increase in the workers' share, but, even so, it would be found that this increase had been achieved without reducing the farmer's share below his pre-war proportion. In the second place, the figures confirm the experience of landowners in that the landlord has received no part of the increased prosperity of farming, whilst, as everyone knows, his expenses of maintenance have enormously increased. Briefly, the situation is that, thanks to the Agricultural Wages Board (and its appointed members may take heart from the fact), the workers have been maintained in the same position as regards their share in the net returns as that in which they were before the war, whilst the farmer has received his share in the increase realised during the past few years, together with that which would have gone to the landlord had the pre-war scale of distribution been maintained. Rents and wages under normal conditions are slow to adjust themselves to changes in farming fortune, and, except in a time of violent economic upheaval, it is right that this should be so, for if the landlord may be regarded as a debenture holder, and labour as a preference shareholder, then the farmer, as the ordinary or deferred shareholder, has to bear the brunt, and if he must take the kicks so also is he entitled to the halfpence.

Turning now from problems in which either the nation generally or whole classes of the industry are concerned, it may be stated that there are many economic problems arising on the farm itself in the solution of which the individual farmer should be able to derive help from the economist. Some of these problems are so simple that their solution should be obvious, but the fact remains that waste in its most easily eliminated forms is constantly to be met with on the farm. The need for the study of the economic use of manual labour has already been referred to in another connection, but, granted that the balance between the employment of land, capital and labour on any farm has been established, cases are continually met with where labour is being mismanaged. It is a not uncommon practice at threshing-time to take the horsemen from their work to assist at threshing, and as this operation can only be performed in dry weather, it may be assumed that the horses might usually be employed on threshing days. With manual labour costing about 7*s.* 6*d.* a day and horses about 5*s.* a day, the advantage of hiring casual labour for threshing, even at high rates of pay, will be obvious when it is remembered that the horseman whose horses are standing idle represents a daily cost for the manual work performed by him of some 18*s*. On a Midland Counties farm, where the maximum possible horse-hours in a certain week in November last were 238, the time actually worked by horses was found to be eighty-seven, owing to threshing operations, and the wastefulness of the labour-management in such a case is obvious. Again, employers in certain cases object to

paying Saturday overtime to men willing to work, because overtime payments are at a higher rate than those for ordinary time, but they overlook entirely the fact that the Agricultural Wages Board provides no overtime payments to the horses, and thus the cheapest horse-labour on the farm is that performed on Saturday afternoon at overtime rates of pay to the horsemen.

Everyone realises, of course, the importance of keeping horses busy, but not everyone thinks how heavily the cost of manual labour is increased by idle horses. The maximum number of working days in a year is 312, a total obviously impossible of attainment in practice. Such records as are available show that the days actually worked by horses on the farm will not usually exceed four-fifths of the maximum. More time may be lost in summer than in winter, a fact not generally realised, and the period of maximum unemployment falls between hay-making and harvest. The busy seasons are, of course, the autumn and the spring, when the preparation of the ground for winter and spring corn is going actively forward. In the year 1918 figures were collected to show the percentage of days worked compared with 'possible days' in each month on four farms distributed pretty evenly over England, and the results, thrown together, are as follows:—

Percentage of Days Worked to Possible Horse-days on Four Farms in 1918.

	%		%
January	67	July	38
February	82	August	65
March	77	September	78
April	74	October	80
May	70	November	67
June	56	December	64

Although the figures represent an average of four farms, it is noteworthy that the results on the individual holdings varied one from another in degree only, and that the months of maximum and minimum employment were the same in every case. The loss of time is far more serious than many people realise. The maximum possible horse-days in the year are 312, and the cost per day of the horses on the above four farms on this basis was 2*s.* 7*d.* whereas, owing to the time lost, the cost on the basis of days worked was 3*s.* 7*d.* Whilst some difference is inevitable, so great a discrepancy as these figures reveal can be avoided by skilful management, and one of the tests of the farmer's efficiency is provided by an examination of the distribution of horse-labour throughout the year on his farm. His cropping and other work should be so contrived as to provide for the uniform utilisation of horse-labour month by month. Under skilful management the differences in the number of days worked by horses from year to year are extraordinarily slight. On an East Midlands farm, employing twenty-three horses, the days worked per horse during the past six years have been as follows:—

Year	1913–14	1914–15	1915–16	1916–17	1917–18	1918–19
Days worked per horse	250·25	247	243	236	243	244·5

It may be noted, in passing, that figures such as those given for the seasonal employment of horse-labour emphasise the need for a study of the place of the agricultural tractor in farm management, for the busiest times of the year synchronise, more or less, with the seasons when the weather is more uncertain and suggest that the application of speedier mechanical power to field operations, in substitution for slower horse-power, would result in economic advantages in certain cases.

In connection with the study of economics on the farm the question of agricultural costings naturally suggests itself. Farmers, as a class, are not accountants and much less are they cost accountants, but this has not deterred many of them from taking part in discussions of farming costs which have been going on in the Press and in the Food Controller's offices for some time past, and the confusion of thought on the question of what cost of production really is which these discussions have revealed is evidence of the need for study and education in costing processes. Few things can be of greater service to the farmer than scientific book-keeping carried out and interpreted with proper understanding, but few things can deceive him more than costing wrongly conducted or misinterpreted. The need for accurate thinking is evidenced in nothing, perhaps, so much as in connection with the question of the valuation of the raw materials grown on the farm, the hay, straw, roots, pasturage, &c., produced for home consumption in the process of manufacturing milk and meat. There can be only one basis of value possible, namely, their cost to the farmer, but it is contended, almost universally, that their market price should be substituted for the sums that he has actually paid for them. As a matter of fact, the bulky feeding stuffs usually produced and consumed at home rarely have any market value at all. A market value is one that can be realised in the market. Thus, corn, meat, and certain other commodities have clearly market value because they are always saleable, but if all the farmers in the country decided to sell their mangolds they would find that the market for mangolds is non-existent, and that the prices quoted in market reports represent a few deals to satisfy an infinitesimal demand. The same is true of straw and, in a slightly less degree, of hay in normal times. Even if the difficulty of fixing the market prices of certain products, such as turnips, straw, or hay, be ignored, and if it be assumed that there be a free market in such things, a fuller consideration of what the farmer really does in feeding them to his stock will show how inapplicable such values are to his case. The market value of an article is the figure at which a willing buyer and a willing seller can agree to do business. The farmer who contends that he is justified in 'selling' his roots or hay to his stock is selling them, in point of fact, to himself, and seeing that there is only one party to the transaction there can be no market and, consequently, no market price. In the majority of cases each of these things is grown because the farmer has need of them in the production of the article or articles of food towards which his management is directed. If he could buy them more cheaply than he can grow them he would surely do so, but to regard himself as a merchant instead of as a manufacturer, and

then to trade with one department of his farm against another is to involve himself in paper transactions which have no foundation in fact, and which may lead to disastrous conclusions. To take, for example, the cost of milk production. It is usual to argue that hay consumed by the cow should be charged at its market price. It may well be that in consequence of a temporary or of a local demand it will pay a farmer better to sell hay rather than to produce milk, and one of the main functions of book-keeping is to enable him to make a decision on such points as this. But he cannot expect to have it both ways; if he sells hay he cannot produce milk, and *vice versa*. Many farmers contract at summer prices for their winter's supply of feeding stuffs, but a man who has bought linseed cake at a pound per ton less than the price current at the time when he is consuming it would hardly think of charging it to bullocks at any other price than that which he actually paid, and it is this figure, the actual cost to him, which must be the measure of the value of all raw materials, whether they be bought in the market, or whether, for the sake of convenience and economy, they be grown on the farm.

Lastly, I want to urge, and particularly before a gathering such as this, the importance of agricultural economics in agricultural education. The fact is realised, no doubt, by many teachers, but until a sufficient body of data bearing on the study of farm management can be made available to them it is impossible for them to give to the teaching of practical agriculture that solid economic basis which is fundamental, and the teacher is driven to include in his instruction much to which the economic test has never been applied and to exclude more for which no basis for teaching exists at all. Given the requisite body of information it would not only be possible but necessary to recast the whole foundations upon which the teaching of practical agriculture rests.

I am not one of the few who appear to derive satisfaction from making comparisons unfavourable to British agriculture with that of other countries, but, when we look at the work which is being done in the United States, in Italy, Germany, Switzerland, and even in Russia before the War, it is surprising to reflect that the agriculturists of the nation which produced Adam Smith, Ricardo, and John Stuart Mill should have been so slow to realise the need for a fuller organisation for the study of agricultural economics.

CONFERENCE OF DELEGATES OF CORRESPONDING SOCIETIES.

THE MESSAGE OF SCIENCE.

ADDRESS BY

SIR RICHARD GREGORY,

PRESIDENT OF THE CONFERENCE.

It is just forty years ago, at the York Meeting in 1881, that a Committee was appointed 'to arrange for a conference of delegates from scientific societies to be held at the annual meetings of the British Association, with a view to promote the interests of the societies represented by inducing them to undertake definite systematic work on a uniform plan.' The Association had been in existence for fifty years before it thus became a bond of union between local scientific societies in order to secure united action with regard to common interests. Throughout the whole period of ninety years it has been concerned with the advancement and diffusion of natural knowledge and its applications. The addresses and papers read before the various sections have dealt with new observations and developments of scientific interest or practical value; and, as in scientific and technical societies generally, questions of professional status and emolument have rarely been discussed. The port of science—whether pure or applied—is free, and a modest yawl can find a berth in it as readily as a splendid merchantman, provided that it has a cargo to discharge. Neither the turmoil of war nor the welter of social unrest have prevented explorers of uncharted seas from crossing the bar and bringing their argosies to the quayside, where fruits and seeds, rich ores and precious stones have been piled in profusion for the creation of wealth, the comforts of life, or the purpose of death, according as they are selected and used.

All that these pioneers of science have asked for is for vessels to be chartered to enable them to make voyages of discovery to unknown lands. Many have been private adventurers, and few have shared in the riches they have brought into port. Corporations and Governments are now eager to provide ships which will bring them profitable freights, and to pay bounties to the crews, but this service is dominated by the commercial spirit which expects immediate returns for investments, and mariners who enter it are no longer free to sail in any direction they please or to enter whatever creek attracts them. The purpose is to secure something of direct profit or use, and not that of discovery alone, by which the greatest advances of science have hitherto been achieved.

When science permits itself to be controlled by the spirit of profitable application it becomes merely the galley-slave of short-sighted commerce. Almost all the investigations upon which modern industry has been built would have been put aside at the outset if the standard of immediate practical value had been applied to them. To the man of science discoveries signify extensions of the field of work, and he usually leaves their exploitation to prospectors who follow him. His motives are intellectual advancement, and not the production of something from which financial gain may be secured. For generations he

has worked in faith purely for the love of knowledge, and has enriched mankind with the fruits of his labours; but this altruistic attribute is undergoing a change. Scientific workers are beginning to ask what the community owes to them, and what use has been made of the talents entrusted to it. They have created stores of wealth beyond the dreams of avarice, and of power unlimited, and these resources have been used to convert beautiful countrysides into grimy centres of industrialism, and to construct weapons of death of such diabolical character that civilised man ought to hang his head in shame at their use.

Mankind has, indeed, proved itself unworthy of the gifts which science has placed at its disposal, with the result that squalid surroundings and squandered life are the characteristics of modern Western civilisation, instead of social conditions and ethical ideals superior to those of any other epoch. Responsibility for this does not lie with scientific discoverers, but with statesmen and democracy. Like the gifts of God, those of science can be made either a blessing or a curse, to glorify the human race or to destroy it; and upon civilised man himself rests the decision as to the course to follow. With science as an ally, and the citadels of ignorance and self as the objective, he can transform the world, but if he neglects the guidance which knowledge can give, and prefers to be led by the phrases of rhetoricians, this planet will become a place of dust and ashes.

Unsatisfactory social conditions are not a necessary consequence of the advance of science, but of incapacity to use it rightly. Whatever may be said of captains of industry or princes of commerce, scientific men themselves cannot be accused of amassing riches at the expense of labour, or of having neglected to put into force the laws of healthy social life. Power—financial and political—has been in the hands of people who know nothing of science, not even that of man himself, and it is they who should be arraigned at the bar of public justice for their failure to use for the welfare of all the scientific knowledge offered to all. Science should dissociate itself entirely from those who have thus abused its favours, and not permit the public to believe it is the emblem of all that is gross and material and destructive in modern civilisation. There was a time when intelligent working men idealised science; now they mostly regard it with distrust or are unmoved by its aims, believing it to be part of a soul-destroying economic system. The obligation is upon men of science to restore the former feeling by removing their academic robes and entering into social movements as citizens whose motives are above suspicion and whose knowledge is at the service of the community for the promotion of the greatest good. The public mind has yet to understand that science is the pituitary body of the social organism, and without it there can be no healthy growth in modern life, mentally or physically.

This Conference of Delegates provides the most appropriate platform of all those offered by the British Association from which a message of exhortation may be given. There are now 130 Corresponding Societies of the Association, with a total membership of about 52,000, and their representatives should every year go back not only strong with zeal for new knowledge, but also as ministers filled with the sense of duty to inspire others to trust in it. In mechanics work is not considered to be done until the point of application of the force is moved; and knowledge, like energy, is of no practical value unless it is dynamic. The scientific society which shuts itself up in a house where a favoured few can contemplate its intellectual riches is no better than a group of misers in its relations to the community around it. The time has come for a crusade which will plant the flag of scientific truth in a bold position

in every province of the modern world. If you believe in the cause of disciplined reason you will respond to the call and help to lift civilised man out of the morass in which he is now struggling, and set him on sound ground with his face toward the light.

It is not by discoveries alone, and the records of them in volumes rarely consulted, that science is advanced, but by the diffusion of knowledge and the direction of men's minds and actions through it. In these democratic days no one accepts as a working social ideal Aristotle's view of a small and highly cultivated aristocracy pursuing the arts and sciences in secluded groves and maintained by manual workers excluded from citizenship. Artisans to-day have quite as much leisure as members of professional classes, and science can assist in encouraging the worthy employment of it. This end can be attained by co-operative action between local scientific societies and representative organisations of labour. There should be close association and a common fellowship, and no suggestion of superior philosophers descending from the clouds to dispense gifts to plebeian assemblies. Above all, it should be remembered that a cause must have a soul as well as a body. The function of a mission-hall is different from that of a cinema-house or other place of entertainment, and manifestations of the spirit of science are more uplifting than the most instructive descriptive lectures.

Science needs champions and advocates, in addition to actual makers of new knowledge and exponents of it. There are now more workers in scientific fields than at any other time, yet relatively less is done to create enthusiasm for their labour and regard for its results than was accomplished fifty years ago. Every social or religious movement passes through like stages, from that of fervent belief to formal ritual. In science specialisation is essential for progress, but the price which has to be paid for it is loss of contact with the general body of knowledge. Concentration upon any particular subject tends to make people indifferent to the aims and work of others; for, while high magnifying powers enable minute details to be discerned, the field of vision is correspondingly narrowed, and the relation of the structure as a whole to pulsating life around it is unperceived.

As successful research is now necessarily limited for the most part to complex ideas and intricate details requiring special knowledge to comprehend them, very special aptitude is required to present it in such a way as will awaken the interest of people familiar only with the vocabulary of everyday life. In the scientific world the way to distinction is discovery, and not exposition, and rarely are the two faculties combined. Most investigators are so closely absorbed in their researches that they are indifferent as to whether people in general know anything of the results or not. In the strict sense of the word, science can never be popular, and its pure pursuit can never pay, but where one person will exercise his intelligence to understand the description of a new natural fact or principle a thousand are ready to admire the high purpose of a scientific quest and reverence the disinterested service rendered by it to humanity. The record of discovery or description of progress is, therefore, only one function of a local scientific society; beyond this is the duty of using the light of science to reveal the dangers of ignorance in high as well as in low places. Though in most societies there is only a small nucleus of working members, the others are capable of being interested in results achieved, and a few may be so stimulated by them as to become just and worthy knights of science, ready to remove any dragons which stand in the way of human progress, and continually upholding the virtues of their mistress.

Every local scientific society should be a training ground for these Sir Galahads, and an outpost of the empire of knowledge. The community should look to it for protection from dangers within and without the settlement, and for assistance in pressing further forward into the surrounding woods of obscurity. At present it is unusual for this civic responsibility to be accepted by a scientific society, with the result that local movements are undertaken without the guidance necessary to make them successful. A local scientific society should be the natural body for the civic authority to consult before any action is taken in which scientific knowledge will be of service. It should be to the city or county in which it is situated what the Royal Society is to the State, and not a thing apart from public life and affairs. As an example of what a local society may usefully do, the action taken by the Manchester Field Naturalists' and Archæologists' Society several years ago may be mentioned. The Society appointed a Committee for the purpose of promoting the planting of trees and shrubs in Manchester and its immediate suburbs. The idea commended itself to the Corporation, and the Committee obtained advice as to the best trees for open spaces in the district, shrubs for tubs and boxes, and tree culture in towns generally. This is the kind of guidance which a scientific society should be particularly competent to give, and which the community has a right to expect from it. Many similar questions continually arise in which ascertained knowledge can be used for the promotion of healthy individual and social life, and if scientific societies are indifferent to them they neglect their best opportunities of playing a strong part in the scheme of human progress.

When wisdom is justified of her children, and local scientific societies are no longer esoteric circles, but effective groups of enlightened citizens of all classes, they will provide the touchstone by which fact is distinguished from assertion and promise from performance. As the sun draws into our system all substantial bodies which come within its sphere of influence, while the pressure of sunlight drives away the fine dust which would tend to obscure one body from another, so a local scientific society possesses the power of attracting within itself all people of weight in the region around it and of dispersing the mist and fog which commonly prevail in the social atmosphere. Thus may the forces of modern civilisation, moral and material, be brought together, and an allied plan of campaign instituted against the armies of ignorance and sloth. The service is that of truth, the discipline that of scientific investigation, and the unifying aim human well-being. Kingsley long ago expressed the democratic basis upon which this fellowship is founded. 'If,' he said, 'you want a ground of brotherhood with men, not merely in these islands, but in America, on the Continent—in a word, all over the world—such as rank, wealth, fashion, or other artificial arrangements of the world cannot give and cannot take away; if you want to feel yourself as good as any man in theory, because you are as good as any man in practice, except those who are better than you in the same line, which is open to any and every man, if you wish to have the inspiring and ennobling feeling of being a brother in a great freemasonry which owns no difference of rank, of creed, or of nationality—the only freemasonry, the only International League which is likely to make mankind (as we all hope they will be some day) one—then become men of science. Join the freemasonry in which Hugh Miller, the poor Cromarty stonemason, in which Michael Faraday, the poor bookbinder's boy, became the companions and friends of the noblest and most learned on earth, looked up to by them not as equals merely, but as teachers and guides, because philosophers and discoverers.'

When Kingsley delivered this message artisans were crowding in thousands to lectures in Manchester and other populous places by leaders in the scientific world of that time. Labour then welcomed science as its ally in the struggle for civil rights and spiritual liberty. That battle has been fought and won, and subjects in bitter dispute fifty years ago now repose in the limbo of forgotten things. There is no longer a conflict between religion and science, and labour can assert its claims in the market-place or council house without fear of repression. Science is likewise free to pursue its own researches and apply its own principles and methods within the realm of observable phenomena, and it does not desire to usurp the functions of faith in sacred dogmas to be perpetually retained and infallibly declared. The Royal Society of London was founded for the extension of *natural* knowledge in contra-distinction to the *supernatural*, and it is content to leave priests and philosophers to describe the world beyond the domain of observation and experiment. When, however, phenomena belonging to the natural world are made subjects of supernatural revelation or uncritical inquiry, science has the right to present an attitude of suspicion towards them. Its only interest in mysteries is to discover the natural meaning of them. It does not need messages from the spirit world to acquire a few elementary facts relating to the stellar universe, and it must ask for resistless evidence before observations contrary to all natural law are accepted as scientific truth. If there are circumstances in which matter may be divested of the property of mass, fairies may be photographed, lucky charms may determine physical events, magnetic people disturb compass needles, and so on, by all means let them be investigated, but the burden of proof is upon those who believe in them and every witness will be challenged at the bar of scientific opinion.

We do not want to go back to the days when absolute credulity was inculcated as a virtue and doubt punished as a crime. It is easy to find in works of uncritical observers of mediæval times most circumstantial accounts of all kinds of astonishing manifestations, but we are not compelled to accept the records as scientifically accurate and to provide natural explanations of them. We need not doubt the sincerity of the observer even when we decline to accept his testimony as scientific truth. The maxim that "Seeing is believing" may be sound enough doctrine for the majority of people, but it is insufficient as a principle of scientific inquiry. For thousands of years it led men to believe that the earth was the centre of the universe, with the sun and other celestial bodies circling round it, and controlling the destiny of man, yet what seemed obvious was shown by Copernicus to be untrue. This was the beginning of the liberation of human life and intellect from the maze of puerile description and philosophic conception. Careful observation and crucial experiment later took the place of personal assertion and showed that events in Nature are determined by permanent law and are not subject to haphazard changes by supernatural agencies. When this position was gained by science, belief in astrology, necromancy, and sorcery of every kind began to decline, and men learned that they were masters of their own destinies. The late War is responsible for a recrudescence of these mediæval superstitions, but if natural science is true to the principles by which it has advanced it will continue to bring to bear upon them the piercing light by which civilised man was freed from their baleful consequences.

There is abundant need for the use of the intellectual enlightenment which science can supply to counteract the ever-present tendency of humanity to revert to primitive ideas. Fifty years of compulsory education are but a moment in the history of man's development, and their influence is as nothing

in comparison with instincts derived from our early ancestors and traditions of more recent times grafted upon them. So little is known of science that to most people old women's tales or the single testimony of a casual onlooker are as credible as the statements and conclusions of the most careful observers. Where exact knowledge exists, however, to place opinion by the side of fact is to blow a bubble into a flame. Within its own domain science is concerned not with belief—except as a subject of inquiry—but with evidence. It claims the right to test all things in order to be able to hold fast to that which is good. It declines to accept popular beliefs as to thunderbolts; living frogs and toads embedded in blocks of coal or other hard rock without an opening, though the rock was formed millions of years ago and all fossils found in it are crushed as flat as paper; the inheritance of microbic diseases; the production of rain by explosions when the air is far removed from its saturation point; the influence of the moon on the weather or of underground water upon a twig held by a dowser, and dozens of like fallacies, solely because when weighed in the balance they have been found wanting in scientific truth. Its only interest in mysteries is that of inquiring into them and finding a natural reason for them. Mystery is thus not destroyed by knowledge but removed to a higher plane.

Never let it be acknowledged that science destroys imagination, for the reverse is the truth. 'The Gods are dead,' said W. E. Henley.

'The world, a world of prose,
Full-crammed with facts, in science swathed and sheeted,
Nods in a stertorous after-dinner doze!
Plangent and sad, in every wind that blows
Who will may hear the sorry words repeated :—
"The Gods are dead."'

It is true that the old idols of wood and stone are gone, but far nobler conceptions have taken their place. The universe no longer consists of a few thousand lamps lit nightly by angel torches, but of millions of suns moving in the infinite azure, into which the mind of man is continually penetrating further. Astronomy shows that realms of celestial light exist where darkness was supposed to prevail, while scientific imagination enables obscure stars to be found which can never be brought within the sense of human vision, the invisible lattice work of crystals to be discerned, and the movements of constituent particles of atoms to be determined as accurately as those of planets around the sun. The greatest advances of science are made by the disciplined use of imagination; but in this field the picture conceived is always presented to Nature for approval or rejection, and her decision upon it is final. In contemporary art, literature, and drama imagination may be dead, but not in science, which can provide hundreds of arresting ideas awaiting beautiful expression by pen and pencil. It has been said that the purpose of poetry is not truth, but pleasure; yet, even if this definition be accepted, we submit that insight into the mysteries of Nature should exalt, rather than repress, the poetic spirit, and be used to enrich verse, as it was by some of the world's greatest poets—Lucretius, Dante, Milton, Goethe, Tennyson, and Browning. With one or two brilliant exceptions, popular writers of the present day are completely oblivious to the knowledge gained by scientific study, and unmoved by the message which science is alone able to give. Unbounded riches have been placed before them, yet they continue to rake the muck-heap of animal passions for themes of composition. Not by their works shall we become 'children of light,' but by the indomitable spirit of man ever straining upwards to reach the stars.

Where there is ignorance of natural laws all physical phenomena are referred to supernatural causes. Disease is accepted as Divine punishment to be met by prayer and fasting, or the act of a secret enemy in communion with evil spirits. Because of these beliefs thousands of innocent people were formerly burnt and tortured as witches and sorcerers, while many thousands more paid in devastating pestilences the penalty which Nature inevitably exacts for crimes against her. In one sense it may be said that the human race gets the diseases it deserves; but the sins are those of ignorance and neglect of physical laws rather than against spiritual ordinances. Plague is not now explained by supposed iniquities of the Jews or conjunctions of particular planets, but by the presence of an organism conveyed by fleas from rats; malaria and yellow fever are conquered by destroying the breeding places of mosquitoes; typhus fever by getting rid of lice; typhoid by cleanliness; tuberculosis by improved housing; and most like diseases by following the teachings of science concerning them. Though the mind does undoubtedly influence the resistance of the body to invasion by microbes, it cannot create the specific organism of any disease, and the responsibility of showing how to keep such germs under control, and prevent, therefore, the poverty and distress due to them, is a scientific rather than a spiritual duty.

The methods of science are pursued whenever observations are made critically, recorded faithfully, and tested rigidly, with the object of using conclusions based upon them as stepping-stones to further progress. They demand an impartial attitude towards evidence and fearless judgment upon it. These are the principles by which the foundations of science have been laid, and a noble structure of natural knowledge erected upon them. A scientific inquiry is understood to be one undertaken solely with the view of arriving at the truth, and this disinterested motive will always command public confidence. It is poles apart from the spirit in which social and political subjects are discussed : it is the rock against which waves of emotion and storms of rhetoric lash themselves in vain. If political science were guided by the same methods it would present an open mind to all sides of a question, weighing objections to proposals as justly as reasons in support of them, whereas usually it sees only the views of a particular class or party, and cannot be trusted, therefore, to strike a judicial balance. The methods of science should be the methods applied to social problems if sound principles of progress are to be determined. When they are so used a statesman will be judged, as a scientific man is judged, by correct observation, just inference, and verified prediction; in their absence politics will remain stranded on the shifting sands of barter, concession, and expediency.

Democracy may be politically an irrational force, but that is all the more reason why those who direct it should have full knowledge of the possibilities offered by science for construction as well as for destruction. In a chemical research an experiment is not the haphazard mixture of substances made in the hope that something good will come from it, but the deliberate test of consequences which ought to follow if certain ideas are true. So with all scientific experiment : reason is the source of action, and principles are tested by results. Social problems are perhaps more complicated than those of the laboratory, yet the only way to discover solutions of them is to apply scientific standards to the methods used and results obtained. Laws of Nature are merely expressions of our knowledge at a particular epoch, and they are more precise than those of political economy because they are investigated purely from the point of view of progress. If the general laws which constitute the science of sociology are to be discovered and accepted, their study must be

as impartial as that of any other science. 'The discovery of exact laws,' said W. K. Clifford, 'has only one purpose—the guidance of conduct by means of them. The laws of political economy are as rigid as those of gravitation; wealth distributes itself as surely as water finds its level. But the use we have to make of the laws of gravitation is not to sit down and cry "Kismet" to the flowing stream, but to construct irrigation works.'

Organised Labour has on more than one occasion pronounced a benison upon scientific research, and urged that full facilities should be afforded to those who undertake it. Not long ago the American Federation of Labour in Convention assembled resolved 'that a broad programme of scientific and technical research is of major importance to the national welfare,' and in a noteworthy document insisted upon its essential value in the development of industries, increased production, and the general welfare of the workers. The British Labour Party has also stated that it places the 'advancement of science in the forefront of its political programme,' but its manifesto refers particularly to the 'undeveloped science of society' rather than to the science of material things; and whatever Labour may declare officially, it is scarcely too much to say that artisans in general show less active interest in scientific knowledge now than they did fifty years ago. Not by the study of science does a manual worker become a leader among his fellows, but by the discovery of wrongs to be remedied or rights to be established, and by fertility of resource in disputations concerning them. This is natural enough, yet when we remember that many of the greatest pioneers in the fields of pure and applied science were of humble origin it is surprising that Labour makes no effort to keep men of this type within its lodges.

If Trades Unions were true to their title, and not merely wage unions, their members would give as much attention to papers on scientific principles of their industry, craftsmanship, and possible new developments as they do to the consideration of the uttermost they can claim and secure for their members. Not a single labour organisation concerns itself with actual means of industrial progress, but only with the sharing of the profits from processes or machinery devised by others. Labour may express approval of scientific and technical research, but if it wishes to be a creative force it should take part in this work instead of limiting itself to getting the greatest possible advantage from the results. Under present conditions an artisan with original ideas or inventive genius has to go outside the circle of his union to describe his work, and he thus becomes separated from his fellows through no fault of his own. His contributions are judged by a scientific or technical society purely on their merit and without any consideration as to his social position. Labour can never be great until it affords like opportunities to its own original men by accepting and issuing papers upon discoveries of value to science and industry. When it does this, and its publications occupy an honoured place among those of scientific and technical societies, it will be able to command a position in national polity which can never be justly conceded to any organisation concerned solely with the rights and privileges of a single class in the community.

We know, of course, that few workmen can be expected to possess sufficient knowledge and originality to make developments important enough to be recorded in papers for the benefit of science or industry generally, but every such contribution published by a Trade Union or other Labour organisation, federated or otherwise, would do far more to command respect than sheaves of pamphlets upon economic aspects of industry from the point of view of workpeople. If no fundamental or suggestive papers of this kind are forthcoming, or if organised Labour persists in its policy of letting its men of

practical genius find elsewhere the people who know how to appreciate them, it is tacitly acknowledged that others are expected to provide the seeds of industrial developments while Labour concerns itself solely with the distribution of the fruits derived from them.

It is true that some of the leaders of the Labour movement realise that close association with progressive science is essential to the expansion of industry and the consequent provision of wages in the future. What is here urged is that Labour should itself take part in the scientific and industrial research which it acknowledges is necessary for existence, and should show by its own contributions that it possesses the power to produce useful knowledge as well as the dexterity to apply it. The machinery of trade unionism is capable of much more extensive use than that to which it has hitherto been put, and when it is concerned not only with securing 'for the producers by hand or by brain the full fruits of their industry,' but also with the creation of new plantations by its own efforts, no one will be able to doubt its fitness to exercise a controlling influence upon modern industry.'

The Workers' Educational Association has proved that very many artisans are ready to take advantage of opportunities of becoming familiar with the noblest works of literature, science, and art, with the single motive of enriching their outlook upon life. Many more attend classes in economics, and nearly all are in favour of extended facilities for further education, though there is a difference of intention between the Marxian element in Labour and the more impartial supporters of the W.E.A. or of the Co-operative Education Union. 'There is practically no limit,' says Mr. G. D. H. Cole in 'An Introduction to Trade Unionism,' 'to what could be done if there only existed among the national and local leaders of Labour a clear idea of the part which education must play if the working-class is ever to achieve emancipation from the wage system.' To education should be added original research if labour is to signify something more than a class of hewers of wood and drawers of water. The Guild movement represents a step in this direction, but if it signifies merely a return to the mediæval system it can scarcely be so important a factor of general development as its advocates imagine, and it may mean the institution of caste in labour. Such a system no doubt leads to perfection of craftsmanship, and it is to be welcomed as an antidote to the deadening influence of specialised industry; but a caste nation at last becomes stationary, for in each caste a habit of action and a type of mind are established which can only be changed with difficulty. What is wanted to make the race strong is cross-fertilisation, and not in-breeding.'

Local scientific societies should provide a common forum where workers with hand or brain can meet to consider new ideas and discuss judicially the significance of scientific discovery or applied device in relation to human progress. At present such societies are mostly out of touch with these practical aspects of knowledge, and are more interested in prehistoric pottery than in the living world around them. Most of those connected to the British Association are concerned with natural history, but all scientific societies in a district should form a federation to proclaim the message of knowledge from the house-tops. Men are ready to listen to the gospel of science and to believe in its power and its guidance, but its disciples disregard the appeal and are content to let others minister to the throbbing human heart. Civilisation awaits the lead which science can give in the name of wisdom and truth and unprejudiced inquiry into all things visible and invisible, but the missionary spirit which would make men eager to declare this noble message to the world has yet to be created.

This is as true of the British Association itself as it is of local scientific societies. It seems to be forgotten that one of the functions of the Association is to inspire belief and confidence in science as the chief formative factor of modern life, and not only to display discoveries or enable specialists to discuss technical advances in segregated sections. Though members of the Association may be able to live on scientific bread alone, most of the community in any place of meeting need something more spiritual to awaken in them the admiration and belief which beget confidence and hope. They ask for a trumpet-call which will unite the forces of natural and social science, and are unmoved by the parade of trophies of scientific conquests displayed to them. It was the primary purpose of Canon W. V. Harcourt, the chief founder of this Association, and General Secretary from 1831 to 1837, to sound this note for 'the stimulation of interest in science at the various places of meeting, and through it the provision of funds for carrying on research,' and not for 'the discussion of scientific subjects in the sections.' In the course of time these sectional discussions have taken a prominent place in the Association's programme, and rightly so, for they have promoted the advancement of science in many directions; but, while we recognise their value to scientific workers, we plead for something more for the great mass of people outside the section-rooms, for a statement of ideals and of service, of the strength of knowledge and of responsibility for its use. These are the subjects which will quicken the pulse of the community and convert those who hate and fear science and associate it solely with debasing aspects of modern civilisation into fervent disciples of a new social faith upon which a lever made in the workshops of natural knowledge may be placed to move the world.

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